# MONTHLY NOTICES

OF THE

# ROYAL ASTRONOMICAL SOCIETY.

VOL. XL. MARCH 12, 1880. No. 5.


J. R. HIND, Esq., F.R.S., President in the Chair.

The Rev. C. S. Harris, M.A., Mepal Rectory, Ely; and

The Rev. J. J. M. Perry, M.A., St. Paul's Vicarage, Alnwick;

were balloted for and duly elected Fellows of the Society.

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## *Sur la Variation du demi-grand Axe des Orbites planétaires.*

By M. A. De Gasparis.

Mon seul but dans cette communication est de montrer qu'en appliquant les résultats contenus dans ma Note (*Astron. Nach.*, N° 2270), on arrive à des valeurs connues.

À la fin du temps $t$,

$$x_1\ y_1\ z_1,\quad x_1'\ y_1'\ z_1$$

sont les coordonnées, et leurs dérivées par rapport au temps, de la masse troublée $m_1$. Après le temps $dt$, $x_1$ aura une variation $(dx_1)$ (elliptique) plus une correction $c_{x_1}$ due à l'intervalle $dt$ après le temps $t$, et par l'influence de la masse troublante $m_2$. De même $x_1'$ aura une variation $(dx_1') + c_{x_1'}$.

L'on voit donc qu'à la fin du temps $t+dt$

$$x_1 \text{ croît de } (dx_1) + (c_{x_1})_0''\ dt^2,$$

$$x_1' \text{ croît de } (dx_1') + (c_{x_1})_0''\ dt.$$

Ainsi pour $y$ et $z$.

Or, on a

$$\frac{1}{a_1} = \frac{2}{r_1} - \frac{dx_1^2 + dy_1^2 + dz_1^2}{k^2\,(1+m_1)\,dt^2};$$

par la différentiation on trouve

$$\frac{da_1}{2a_1^2\,dt} = \frac{r_1\,dr_1}{r_1^3\,dt} + \frac{dx_1\,dx_1' + dy_1\,dy_1' + dz_1\,dz_1'}{k^2\,(1+m_1)\,dt^2},$$

c'est-à-dire,

$$\frac{da_1}{2a_1^2\,dt} = \frac{x_1\,dx_1 + y_1\,dy_1 + z_1\,dz_1}{r_1^3\,dt} + \frac{dx_1\,dx_1' + dy_1\,dy_1' + dz_1\,dz_1'}{k^2\,(1+m_1)\,dt^2},$$

dans laquelle au lieu de $dx_1\ dx_1'$ il faut mettre $(dx_1) + (c_{x_1})_0''\,dt^2$, et $(dx_1') + (c_{x_1})_0''\,dt$, ainsi pour $y$ et $z$.

Après quelques réductions qui se présentent en tenant compte de ce qui est dû au mouvement elliptique, et par l'évanouissement des termes de second ordre, l'on trouve

$$\frac{da_1}{2a_1^2\,dt} = \frac{dx_1\,(c_{x_1})_0'' + dy_1\,(c_{y_1})_0'' + dz_1\,(c_{z_1})_0''}{k^2\,(1+m_1)\,dt};$$

mais l'on a

$$(c_{x_1})_0'' = m_2 k^2 \left(\frac{x_2 - x_1}{\rho_{12}^3} - \frac{x_2}{r_2^3}\right), \text{ etc.}$$

Donc

$$\frac{da_1}{2a_1^2\,dt} = \frac{m_2}{1+m_1}\left\{\frac{dx_1\,(x_2-x_1) + dy_1\,(y_2-y_1) + dz_1\,(z_2-z_1)}{\rho_{12}^3\,dt} - \frac{dx_1 . x_2 + dy_1 . y_2 + dz_1 . z_2}{r_2^3\,dt}\right\}$$

$$= \frac{m_2}{1+m_1} \cdot \frac{d}{dt}\left\{\frac{1}{\rho_{12}} - \frac{x_1x_2 + y_1y_2 + z_1z_2}{r_2^3}\right\},$$

en faisant varier dans cette dernière seulement les coordonnées de $m_1$.

(Voyez l'excellent mémoire de M. Lespiault, *Théorie géométrique de la Variation des Éléments des Planètes*, 1868, Paris, Gauthier-Villars.)

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*On a Form of Reading Microscope which appears to combine most of the Advantages of the German and English Forms.* By E. J. Stone, M.A., F.R.S.

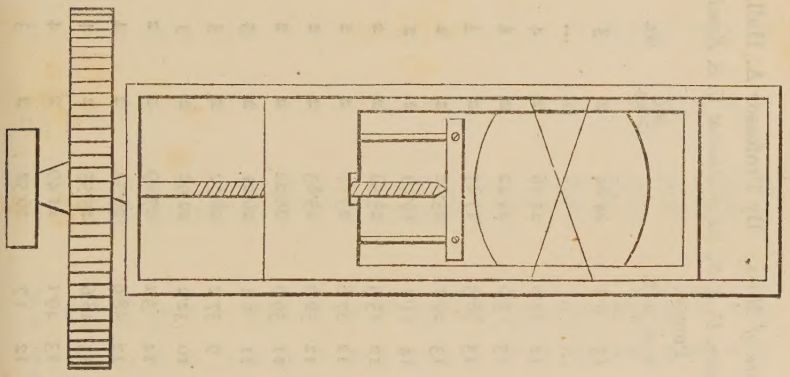
The chief advantage of the English form appears to consist in the sensible coincidence of the axis of the screw of measurement with the line along which the measures are made.

The chief defects appear to be connected with the nature of the bearing from which measurements are made. This bearing is much exposed, and any alteration of its state during the observations directly affects to its full extent the readings.

The chief advantages of the German form appear to arise from the nature of the bearing against which the screw abuts, and from which the measures are taken, and the protection of this bearing from dirt by its inclosure within the microscope.

The chief defect of this form appears to be that the axis of the measuring screw is far removed from the line of measurement, and that any play in the position of the screw produces very sensible errors along the line of measurement.

If these views are correct, there is no great difficulty in making a microscope, as shown in the figure, which shall combine the advantages of both systems.



I have furnished Mr. Simms with a plan which appeared to satisfy the necessary requirements, and he has kindly had a Microscope constructed for exhibition at the Meeting.* I should be glad to hear of objections to the form suggested, with a view to improvements.

* The Microscope was exhibited at the January Meeting.—Ed.

## Observations of the Satellites of Mars. By Professor A. Hall.

*(Communicated by Rear-Admiral John Rodgers, U. S. N., Superintendent U. S. Naval Observatory.)*

| Date. | Wash. M. T. | | PHOBOS. $p$ | No. Comp. | t. | Wash. M. T. | | $s$ | No. Comp. | Wt. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | h | m | ° | | | h | m | ″ | | | |
| 1879 Oct. 12 | 13 | 3·5 | 52·12 | 4 | 3 | 13 | 9·5 | 24·24 | 2 | 3 | |
| 15 | 13 | 53·5 | 234·53 | 2 | 1 | ... | ... | ... | ... | ... | Extremely faint, sky murky. |
| 16 | 12 | 32·0 | 235·71 | 4 | 4 | 12 | 37·0 | 25·46 | 2 | 4 | Very bright. |
| 16 | 12 | 59·0 | 231·45 | 4 | 4 | 13 | 5·0 | 24·42 | 2 | 4 | |
| 16 | 13 | 33·5 | 225·61 | 3 | 3 | 13 | 38·5 | 17·87 | 2 | 3 | Faint. |
| 19 | 13 | 14·2 | 54·36 | 4 | 2 | 13 | 20·7 | 25·92 | 2 | 2 | |
| 19 | 14 | 4·7 | 46·61 | 4 | 2 | 14 | 11·2 | 19·49 | 2 | 2 | Very faint. |
| 20 | 12 | 10·3 | 55·35 | 4 | 2 | 12 | 15·3 | 25·54 | 2 | 2 | Faint, sky murky. |
| 20 | 12 | 41·3 | 50·38 | 4 | 2 | 12 | 47·8 | 23·77 | 2 | 2 | |
| 23 | 12 | 47·5 | 234·54 | 4 | 2 | 12 | 56·0 | 25·83 | 2 | 2 | |
| 24 | 11 | 54·1 | 234·97 | 4 | 2 | 11 | 59·6 | 26·22 | 2 | 2 | Faint. |
| 25 | 11 | 1·6 | 231·58 | 4 | 3 | 11 | 6·1 | 26·21 | 2 | 3 | |
| 26 | 9 | 52·2 | 233·81 | 4 | 3 | 9 | 57·2 | 26·72 | 2 | 3 | Visible with Mars in field. |
| 26 | 10 | 26·7 | 228·16 | 4 | 3 | 10 | 32·2 | 22·92 | 2 | 3 | |
| 2 | 13 | 56·1 | 54·74 | 4 | 2 | 14 | 3·1 | 27·09 | 2 | 2 | Faint, moonlight. |
| 3 | 12 | 54·1 | 53·88 | 4 | 4 | 12 | 58·6 | 26·34 | 2 | 4 | |
| 3 | 13 | 15·1 | 49·77 | 4 | 4 | 13 | 18·6 | 25·55 | 2 | 4 | |
| 3 | 13 | 44·1 | 44·76 | 4 | 4 | 13 | 49·1 | 21·49 | 2 | 4 | |
| 4 | 11 | 57·2 | 52·13 | 4 | 4 | 12 | 1·7 | 26·52 | 2 | 3 | |

| Date. | Wash. M. T. | | $p$ | No. Comp. | Wt. | Wash. M. T. | | $s$ | No. Comp. | Wt. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | h | m | ″ | | | h | m | | | | |
| Nov. 4 | 12 | 52·7 | 41·48 | 4 | 4 | 12 | 58·7 | 18·03 | 2 | 4 | |
| 6 | 10 | 15·8 | 48·33 | 4 | 2 | 10 | 21·3 | 24·13 | 2 | 2 | |
| 6 | 13 | 48·3 | 232·37 | 4 | 3 | 13 | 53·8 | 26·52 | 2 | 3 | |
| 7 | 8 | 57·8 | 51·00 | 4 | 2 | 9 | 3·8 | 25·83 | 2 | 2 | |
| 7 | 12 | 56·3 | 229·11 | 4 | 3 | 13 | 1·3 | 25·67 | 2 | 3 | |
| 12 | 11 | 24·6 | 50·80 | 4 | 2 | 11 | 30·1 | 25·21 | 2 | 2 | Windy. |
| 13 | 10 | 4·1 | 54·05 | 4 | 3 | 10 | 8·1 | 25·89 | 2 | 3 | |
| 14 | 9 | 2·1 | 53·19 | 4 | 3 | 9 | 6·6 | 25·93 | 2 | 3 | |
| 14 | 12 | 19·3 | 238·93 | 4 | 2 | 12 | 27·1 | 23·91 | 2 | 2 | Clouds. |
| 14 | 13 | 6·6 | 231·28 | 4 | 3 | 13 | 13·1 | 25·86 | 2 | 3 | |
| 15 | 11 | 48·4 | 233·42 | 4 | 4 | 11 | 53·4 | 26·43 | 2 | 4 | |
| 16 | 11 | 0·8 | 232·13 | 4 | 4 | 11 | 5·3 | 25·91 | 2 | 4 | Visible with Mars in field. |
| 20 | 10 | 38·5 | 51·29 | 4 | 3 | 10 | 44·0 | 24·76 | 2 | 3 | |
| 21 | 9 | 25·5 | 53·72 | 4 | 3 | 9 | 29·5 | 25·15 | 2 | 3 | |
| 21 | 13 | 21·5 | 232·26 | 4 | 3 | 13 | 27·0 | 25·17 | 2 | 3 | No illumination |
| 22 | 8 | 5·6 | 56·95 | 4 | 3 | 8 | 10·6 | 24·37 | 2 | 3 | |
| 23 | 11 | 27·7 | 229·21 | 4 | 3 | 11 | 32·7 | 24·57 | 2 | 3 | |
| 24 | 6 | 41·3 | 47·46 | 4 | 2 | 6 | 46·3 | 23·52 | 2 | 2 | |
| 24 | 10 | 11·3 | 231·84 | 4 | 4 | 10 | 16·8 | 24·85 | 2 | 4 | |
| 30 | 7 | 59·7 | 48·06 | 4 | 3 | 8 | 5·2 | 23·07 | 2 | 3 | |
| Dec. 1 | 6 | 59·8 | 48·84 | 4 | 3 | 7 | 4·8 | 22·86 | 2 | 3 | Faint. |
| 1 | 10 | 46·8 | 228·91 | 4 | 3 | 10 | 50·8 | 22·94 | 2 | 3 | Faint. |

## Deimos.

| Date. | Wash. M. T. h | m | $p$ ° | No. Comp. | Wt. | Wash. M. T. h | m | " | No. Comp. | Wt. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1879 Oct. 13 | 12 | 10·8 | 57·11 | 4 | 2 | 12 | 17·3 | 59·47 | 2 | 2 | Very faint, haze. |
| 13 | 13 | 37·8 | 53·85 | 4 | 2 | 13 | 47·3 | 63·45 | 2 | 2 | Very faint, haze. |
| 15 | 10 | 36·0 | 235·78 | 4 | 2 | 10 | 41·5 | 63·93 | 2 | 2 | Faint. |
| 15 | 11 | 52·5 | 232·26 | 4 | 2 | 12 | 0·0 | 62·81 | 2 | 2 | Visible with Mars in field. |
| 15 | 13 | 26·5 | 228·66 | 4 | 2 | 13 | 32·5 | 56·90 | 2 | 2 | Faint, haze. |
| 16 | 12 | 45·0 | 249·30 | 4 | 4 | 12 | 52·0 | 37·55 | 2 | 4 | |
| 16 | 13 | 19·0 | 246·03 | 4 | 3 | 13 | 25·3 | 42·93 | 2 | 3 | Very faint, haze. |
| 20 | 11 | 5·8 | 236·74 | 4 | 3 | 11 | 9·3 | 63·29 | 2 | 3 | |
| 20 | 12 | 54·3 | 232·76 | 4 | 3 | 12 | 59·3 | 64·74 | 2 | 3 | |
| 23 | 13 | 6·5 | 60·17 | 4 | 2 | 13 | 13·5 | 54·61 | 2 | 2 | |
| 24 | 11 | 40·6 | 219·64 | 4 | 2 | 11 | 45·6 | 40·08 | 2 | 2 | Faint. |
| 25 | 10 | 52·6 | 240·46 | 4 | 3 | 10 | 57·6 | 57·60 | 2 | 3 | |
| 25 | 13 | 1·6 | 234·13 | 4 | 3 | 13 | 7·1 | 66·81 | 2 | 3 | |
| 29 | 10 | 9·8 | 229·59 | 4 | 1 | 10 | 18·8 | 62·83 | 2 | 1 | Extremely faint. |
| Nov. 2 | 14 | 28·1 | 66·60 | 4 | 2 | 14 | 42·4 | 51·48 | 2 | 2 | Faint, clouds and moonlight. |
| 3 | 10 | 22·6 | 230·48 | 4 | 4 | 10 | 27·1 | 65·98 | 2 | 4 | Just visible with Mars in field. |
| 3 | 13 | 3·1 | 220·62 | 4 | 4 | 13 | 8·1 | 49·33 | 2 | 4 | |
| 3 | 14 | 1·1 | 215·59 | 4 | 4 | 14 | 7·6 | 39·20 | 2 | 4 | |

| Date. | Wash. M. T. h | m | $p$ ° | No. Comp. | Wt. | Wash. M. T. h | m | $s$ ″ | No. Comp. | Wt. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Nov. 4 | 11 | 43·2 | 246·44 | 4 | 3 | 11 | 49·7 | 48·86 | 2 | 3 | |
| 4 | 13 | 4·7 | 240·69 | 4 | 4 | 13 | 11·2 | 59·68 | 2 | 4 | |
| 6 | 10 | 1·8 | 62·76 | 4 | 2 | 10 | 6·8 | 55·54 | 2 | 2 | |
| 6 | 12 | 4·8 | 55·65 | 4 | 3 | 12 | 10·3 | 65·27 | 2 | 3 | |
| 6 | 13 | 34·3 | 51·56 | 4 | 3 | 13 | 39·8 | 65·88 | 2 | 3 | |
| 7 | 9 | 10·8 | 210·32 | 4 | 2 | 9 | 15·8 | 36·65 | 2 | 2 | Faint. |
| 10 | 9 | 17·5 | 48·55 | 4 | 2 | 9 | 22·0 | 63·13 | 2 | 2 | Clock fails and acts badly. |
| 12 | 7 | 54·9 | 223·29 | 4 | 2 | 8 | 12·1 | 55·12 | 2 | 2 | Clouds, clouds. |
| 12 | 10 | 15·6 | 209·29 | 4 | 2 | ... | ... | ... | ... | ... | Very faint. |
| 13 | 7 | 42·6 | 246·92 | 4 | 2 | 7 | 50·6 | 49·22 | 2 | 2 | |
| 13 | 10 | 13·1 | 236·62 | 4 | 4 | 10 | 18·1 | 64·65 | 2 | 4 | |
| 14 | 12 | 36·6 | 258·08 | 4 | 2 | 12 | 55·3 | 38·14 | 2 | 2 | Clouds. |
| 15 | 9 | 19·9 | 50·82 | 4 | 3 | 9 | 25·9 | 65·30 | 2 | 3 | Visible with Mars in field. |
| 15 | 11 | 35·9 | 43·35 | 4 | 4 | 11 | 41·9 | 55·96 | 2 | 4 | |
| 16 | 11 | 13·3 | 68·73 | 4 | 4 | 11 | 18·3 | 46·93 | 2 | 4 | |
| 18 | 11 | 42·4 | 235·21 | 4 | 2 | ... | ... | ... | ... | ... | Faint, cloudy. |
| 20 | 8 | 6·0 | 58·70 | 4 | 2 | 8 | 12·0 | 60·42 | 2 | 2 | Windy. |
| 20 | 10 | 29·0 | 50·59 | 4 | 3 | 10 | 33·5 | 63·36 | 2 | 3 | |
| 21 | 13 | 10·5 | 64·59 | 4 | 3 | 13 | 15·5 | 52·67 | 2 | 3 | No illumination, lamp fails. |

| Date. | Wash. M. T. h | m | θ | No. Comp. | Wt. | Wash. M. T. h | m | s | No. Comp. | Wt. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Nov. 22 | 7 | 52·1 | 230°43 | 4 | 4 | 7 | 57·6 | 62″71 | 2 | 4 | |
| 23 | 11 | 7·7 | 241·70 | 4 | 2 | 11 | 22·2 | 57·17 | 2 | 2 | Clouds. |
| 24 | 6 | 51·8 | 44·67 | 4 | 3 | 6 | 55·8 | 56·92 | 2 | 3 | |
| 29 | 6 | 49·1 | 48·29 | 4 | 3 | 6 | 55·1 | 58·50 | 2 | 3 | Faint. |
| 30 | 9 | 53·2 | 61·23 | 4 | 3 | 9 | 58·2 | 53·70 | 2 | 3 | |
| Dec. 1 | 7 | 11·8 | 214·59 | 4 | 3 | 7 | 17·8 | 41·59 | 2 | 3 | Very faint. |
| 12 | 11 | 47·8 | 232·56 | 4 | 3 | 11 | 55·8 | 51·46 | 1 | 3 | Faint. |
| 16 | 7 | 37·2 | 228·55 | 4 | 2 | 7 | 44·2 | 50·37 | 1 | 2 | Very faint. |
| 18 | 5 | 55·2 | 44·77 | 4 | 3 | 5 | 59·7 | 47·68 | 2 | 3 | Faint, but distinctly seen. |

The outer satellite was found immediately on the first night it was looked for, October 10, and very near the position computed from my elements. The sky soon became cloudy, and no observation could be made on that night. On the next clear night, October 12, I had the pleasure of finding the inner satellite, also near the computed position. This satellite was decidedly brighter than I expected to see it. After this day the sky became covered with a thin haze, which made the observations of the satellites difficult, but on October 16, the sky cleared up and the satellites came out remarkably bright. On this day Professor Holden, who was observing the inner satellites of Saturn, made the following note:—

"Phobos easily seen; it is much brighter than I have ever seen Mimas at elongation, and I should say almost, if not quite, as bright as Enceladus is at elongation."

The observations were made in the following manner. In order to avoid sliding the eye-piece, as was done in 1877, a piece of coloured glass, covering one half the field of view, was inserted in the forward end of the eye-piece, near the micrometer wires. It might be better to silver one half the forward lens of the eye-piece, but an attempt to do this did not give a good result. In making the observations, the planet was placed behind the coloured glass, through which the wire could be seen, and, both objects being kept near the centre of the field, the angle of position and the distance were measured by bisecting the disk of the planet and the satellite. In this way the observations were made in much less time than by sliding the eye-piece. Generally, as will be seen, each observation depends on four settings of the position circle, and four measures of the single distance, the No. of Comp. for the distance being divided by 2, to make them conform to those of 1877. The effect of the coloured glass was to give a slightly different adjustment of the focus for the two parts of the field, but this adjustment was always made for the satellite, or the free part of the field. The eye-piece used was one furnished by the makers of the telescope, and gives a magnifying power of about 400. This eye-piece is not quite achromatic, showing some colour near the edges of the field; and in order to compare the measures made with it and the coloured glass, with those made with the achromatic eye-piece used in 1877, and which has no coloured glass, I measured the distance of Mars from several stars which it passed, with both eye-pieces, the measures being so arranged that the motion of the planet was nearly eliminated for the mean of the times. These measures show no constant difference between the two eye-pieces, and I think the measures of last year will be found as accurate as those of 1877.

The observations have been reduced in the same manner as those of 1877, and the preceding values of $p$ and $s$ are corrected for differential refraction and the figure of the disk.

Before comparing the observations with the elements the perturbations of these satellites produced by the Sun were computed,

and for the secular perturbations, I found values agreeing with those given by Mr. Marth (*Astr. Nach.* No. 2280). If we suppose the perturbations to be really effective, the probability that the satellites at the time of their discovery should move in planes coincident with the equator of the planet is very slight. There can be but little doubt that the view taken by Professor Adams, (*Monthly Notices*, November 1879), and also by M. Tisserand, (*Comptes Rendus de l'Académie des Sciences*, Dec. 8, 1879), will prove to be correct: viz. that these satellites are held nearly in the plane of the equator by the slightly elliptical figure of the planet. For this reason I have neglected the secular perturbations. The coefficients of some of the periodical terms depending on the position of the satellite in its orbit may amount to nearly three minutes of arc; but these terms have very short periods and their effect on the observations is nearly insensible. I have therefore, for the present, neglected these also.

In the case of Phobos, the Besselian auxiliary quantities $f$, $F$, $g$, $G$, depending on the position of its orbit-plane, were computed from the elements found from the Washington observations of 1877. A preliminary calculation having shown that the periodic time of this satellite is nearly

$$\begin{array}{rrrr} & \text{h} & \text{m} & \text{s} \\ \text{T} = & 7 & 39 & 13{\cdot}996, \end{array}$$

the values of $u$ were computed for this value of the period. The observations of Deimos were compared directly with the elements.

The following tables give the results of these comparisons. The residuals $\Delta p$ have been converted into errors of longitude $\Delta u$, and for these errors I assume that the equation of condition is of the form

$$x + by + n = 0,$$

in which $x = \Delta u$ for the epoch Nov. 5.0 Greenwich m.t.; $y$ is the variation of $\Delta u$ in ten days, and $n$ is the value of the error in longitude resulting from the observation. Strictly, the weights for these equations should depend on the factor for converting the residuals $\Delta p$ into errors of longitude. This factor varies from 2·9 to 6·2 in the case of Phobos; and from 1·1 to 5·6 for Deimos. But on the other hand, when this factor becomes small the observations were more difficult, and in the present work I have given the weight unity to all the observations, except that of Phobos on Oct. 15, which depends on two comparisons only, made when the satellite was hardly visible; and to this I have given a weight of one-fourth.

The quantities $\Delta p$ and $\Delta s$ are given in the sense, Calculated *minus* Observed place. In the last column are given the residuals found by substituting the resulting values of $x$ and $y$ in the equations of condition.

PHOBOS.

| | $\Delta p$ | $\Delta s$ | Equations of Condition. | Residuals. |
|---|---|---|---|---|
| | ° | ″ | | ° |
| Oct. 12 | +1·31 | +0·54 | $x-2{\cdot}32y-8{\cdot}09=0$ | −7·93 |
| 15 | −2·18 | ... | $\frac{1}{2}x-1{\cdot}01y+6{\cdot}52=0$ | +6·71 |
| 16 | −0·88 | −0·18 | $x-1{\cdot}93y+5{\cdot}14=0$ | +5·59 |
| 16 | −0·20 | +0·07 | $x-1{\cdot}92y+1{\cdot}13=0$ | +1·59 |
| 16 | 0·00 | +1·23 | $x-1{\cdot}92y+0{\cdot}00=0$ | +0·46 |
| 19 | +0·31 | −0·20 | $x-1{\cdot}62y-1{\cdot}75=0$ | −1·06 |
| 19 | +0·78 | +1·89 | $x-1{\cdot}62y-3{\cdot}25=0$ | −2·56 |
| 20 | −0·41 | +0·18 | $x-1{\cdot}53y+2{\cdot}27=0$ | +3·02 |
| 20 | +0·23 | +0·89 | $x-1{\cdot}53y-1{\cdot}21=0$ | −0·46 |
| 23 | +1·04 | +0·17 | $x-1{\cdot}23y-5{\cdot}45=0$ | −4·47 |
| 24 | −0·77 | +0·07 | $x-1{\cdot}13y+4{\cdot}11=0$ | +5·17 |
| 25 | +1·12 | +0·16 | $x-1{\cdot}03y-5{\cdot}87=0$ | −4·74 |
| 26 | −0·13 | −0·22 | $x-0{\cdot}94y+0{\cdot}68=0$ | +1·88 |
| 26 | −0·01 | +0·94 | $x-0{\cdot}93y+0{\cdot}04=0$ | +1·25 |
| Nov. 2 | −0·27 | −0·32 | $x-0{\cdot}22y+1{\cdot}25=0$ | +2·99 |
| 3 | +0·44 | +0·37 | $x-0{\cdot}13y-2{\cdot}01=0$ | −0·20 |
| 3 | +1·00 | +0·94 | $x-0{\cdot}12y-4{\cdot}38=0$ | −2·56 |
| 3 | +0·54 | +1·09 | $x-0{\cdot}12y-1{\cdot}79=0$ | +0·03 |
| 4 | +1·17 | +0·31 | $x-0{\cdot}03y-5{\cdot}35=0$ | −3·46 |
| 4 | +0·32 | +1·92 | $x-0{\cdot}02y-0{\cdot}85=0$ | +1·04 |
| 6 | +1·35 | +1·81 | $x+0{\cdot}16y-5{\cdot}71=0$ | −3·68 |
| 6 | −0·37 | +0·20 | $x+0{\cdot}18y+1{\cdot}64=0$ | +3·69 |
| 7 | +0·68 | +0·79 | $x+0{\cdot}26y-2{\cdot}96=0$ | −0·85 |
| 7 | +0·96 | +0·49 | $x+0{\cdot}28y-4{\cdot}07=0$ | −1·95 |
| 12 | +0·87 | +1·09 | $x+0{\cdot}77y-3{\cdot}55=0$ | −1·06 |
| 13 | +0·60 | +0·23 | $x+0{\cdot}86y-2{\cdot}37=0$ | +0·19 |
| 14 | +1·33 | +0·19 | $x+0{\cdot}96y-5{\cdot}20=0$ | −2·57 |
| 14 | +3·19 | −0·88 | $x+0{\cdot}97y-9{\cdot}18=0$ | −6·54 |
| 14 | +0·27 | +0·29 | $x+0{\cdot}98y-1{\cdot}07=0$ | +1·58 |
| 15 | +1·18 | −0·44 | $x+1{\cdot}07y-4{\cdot}55=0$ | −1·83 |
| 16 | −0·58 | +0·11 | $x+1{\cdot}17y+2{\cdot}26=0$ | +5·05 |
| 20 | +0·43 | +0·72 | $x+1{\cdot}56y-1{\cdot}60=0$ | +1·49 |
| 21 | +0·18 | +0·14 | $x+1{\cdot}66y-0{\cdot}66=0$ | +2·50 |
| 21 | +0·67 | +0·31 | $x+1{\cdot}68y-2{\cdot}48=0$ | +0·70 |
| 22 | +0·92 | −0·29 | $x+1{\cdot}75y-3{\cdot}00=0$ | +0·23 |
| 23 | +0·84 | +0·15 | $x+1{\cdot}87y-3{\cdot}00=0$ | +0·32 |
| 24 | +1·27 | +0·70 | $x+1{\cdot}95y-4{\cdot}39=0$ | −1·01 |
| 24 | +1·18 | +0·04 | $x+1{\cdot}96y-4{\cdot}23=0$ | −0·84 |

| | $\Delta p$ | $\Delta s$ | Equations of Condition. | Residuals. |
|---|---|---|---|---|
| Nov. 30 | $+1^{\circ}\cdot76$ | $+0''\cdot36$ | $x+2\cdot55y-6^{\circ}\cdot53=0$ | $-2^{\circ}\cdot69$ |
| Dec. 1 | $+0\cdot38$ | $+0\cdot25$ | $x+2\cdot65y-1\cdot27=0$ | $+2\cdot64$ |
| 1 | $+0\cdot92$ | $+0\cdot19$ | $x+2\cdot67y-3\cdot08=0$ | $+0\cdot85$ |

DEIMOS.

| | | | | |
|---|---|---|---|---|
| Oct. 13 | $+0^{\circ}\cdot24$ | $+0''\cdot48$ | $x-2\cdot23y-1^{\circ}\cdot24=0$ | $-2^{\circ}\cdot23$ |
| 13 | $+0\cdot34$ | $-0\cdot08$ | $x-2\cdot22y-1\cdot92=0$ | $-2\cdot91$ |
| 15 | $-0\cdot65$ | $-1\cdot04$ | $x-2\cdot04y+3\cdot58=0$ | $+2\cdot58$ |
| 15 | $+0\cdot12$ | $-0\cdot82$ | $x-2\cdot03y-0\cdot63=0$ | $-1\cdot63$ |
| 15 | $-0\cdot07$ | $-1\cdot57$ | $x-2\cdot02y+0\cdot30=0$ | $-0\cdot70$ |
| 16 | $-0\cdot73$ | $+1\cdot35$ | $x-1\cdot93y+1\cdot46=0$ | $+0\cdot46$ |
| 16 | $-0\cdot49$ | $+1\cdot24$ | $x-1\cdot92y+1\cdot28=0$ | $+0\cdot28$ |
| 20 | $-0\cdot26$ | $-0\cdot06$ | $x-1\cdot53y+1\cdot31=0$ | $+0\cdot30$ |
| 20 | $-0\cdot42$ | $-0\cdot83$ | $x-1\cdot52y+2\cdot16=0$ | $+1\cdot15$ |
| 23 | $+1\cdot30$ | $+1\cdot52$ | $x-1\cdot22y-4\cdot71=0$ | $-5\cdot74$ |
| 24 | $-1\cdot91$ | $-1\cdot87$ | $x-1\cdot13y+3\cdot30=0$ | $+2\cdot27$ |
| 25 | $-0\cdot01$ | $+1\cdot03$ | $x-1\cdot03y+0\cdot04=0$ | $-1\cdot00$ |
| 25 | $+0\cdot22$ | $-0\cdot84$ | $x-1\cdot02y-1\cdot09=0$ | $-2\cdot13$ |
| 29 | $-1\cdot19$ | $-2\cdot59$ | $x-0\cdot64y+4\cdot68=0$ | $+3\cdot63$ |
| Nov. 2 | $-1\cdot12$ | $+0\cdot88$ | $x-0\cdot22y+2\cdot87=0$ | $+1\cdot80$ |
| 3 | $-0\cdot37$ | $-1\cdot11$ | $x-0\cdot14y+1\cdot53=0$ | $+0\cdot46$ |
| 3 | $-0\cdot35$ | $+0\cdot23$ | $x-0\cdot12y+0\cdot82=0$ | $-0\cdot25$ |
| 3 | $-1\cdot91$ | $-1\cdot98$ | $x-0\cdot12y+3\cdot13=0$ | $+2\cdot06$ |
| 4 | $-0\cdot42$ | $+1\cdot18$ | $x-0\cdot03y+0\cdot99=0$ | $-0\cdot09$ |
| 4 | $-0\cdot35$ | $+0\cdot82$ | $x-0\cdot03y+1\cdot21=0$ | $+0\cdot13$ |
| 6 | $-0\cdot65$ | $+1\cdot55$ | $x+0\cdot16y+1\cdot55=0$ | $+0\cdot46$ |
| 6 | $-0\cdot28$ | $+1\cdot07$ | $x+0\cdot17y+0\cdot91=0$ | $-0\cdot18$ |
| 6 | $-0\cdot43$ | $+0\cdot22$ | $x+0\cdot18y-1\cdot77=0$ | $+0\cdot68$ |
| 7 | $-1\cdot16$ | $-2\cdot90$ | $x+0\cdot26y+1\cdot27=0$ | $+0\cdot18$ |
| 10 | $-0\cdot62$ | $-0\cdot18$ | $x+0\cdot56y+3\cdot42=0$ | $+2\cdot31$ |
| 12 | $-0\cdot20$ | $-1\cdot70$ | $x+0\cdot75y+0\cdot54=0$ | $-0\cdot57$ |
| 12 | $-2\cdot15$ | ... | $x+0\cdot76y+2\cdot27=0$ | $+1\cdot16$ |
| 13 | $-0\cdot57$ | $+1\cdot69$ | $x+0\cdot85y+1\cdot28=0$ | $+0\cdot17$ |
| 13 | $-0\cdot32$ | $-0\cdot02$ | $x+0\cdot86y+1\cdot18=0$ | $+0\cdot06$ |
| 14 | $-1\cdot65$ | $+1\cdot90$ | $x+0\cdot97y+2\cdot16=0$ | $+1\cdot04$ |
| 15 | $+0\cdot03$ | $-0\cdot56$ | $x+1\cdot06y-0\cdot11=0$ | $-1\cdot23$ |
| 15 | $-0\cdot80$ | $-1\cdot83$ | $x+1\cdot07y+2\cdot10=0$ | $+0\cdot97$ |
| 16 | $-1\cdot15$ | $+1\cdot79$ | $x+1\cdot17y+2\cdot37=0$ | $+1\cdot24$ |

| | $\Delta p$ | $\Delta s$ | Equations of Condition. | Residuals. |
|---|---|---|---|---|
| | ° | ″ | ° | ° |
| Nov. 18 | −0·29 | ... | $x+1{\cdot}37y+1{\cdot}04=0$ | −0·10 |
| 20 | −0·23 | +0·31 | $x+1{\cdot}56y+0{\cdot}79=0$ | −0·35 |
| 20 | −0·22 | −0·33 | $x+1{\cdot}57y+1{\cdot}15=0$ | 0·00 |
| 21 | −1·12 | +1·34 | $x+1{\cdot}68y+2{\cdot}86=0$ | +1·71 |
| 22 | −0·11 | −0·48 | $x+1{\cdot}75y+0{\cdot}38=0$ | −0·77 |
| 23 | −0·15 | −0·29 | $x+1{\cdot}87y-0{\cdot}42=0$ | −1·58 |
| 24 | −0·26 | −1·26 | $x+1{\cdot}95y+0{\cdot}72=0$ | −0·44 |
| 29 | +0·18 | −0·50 | $x+2{\cdot}45y-0{\cdot}57=0$ | −1·75 |
| 30 | −0·75 | +1·04 | $x+2{\cdot}56y+2{\cdot}11=0$ | +0·90 |
| Dec. 1 | −1·09 | +0·81 | $x+2{\cdot}65y+1{\cdot}81=0$ | +0·61 |
| 12 | −0·46 | +1·15 | $x+3{\cdot}77y+1{\cdot}45=0$ | +0·22 |
| 16 | −0·02 | −1·11 | $x+4{\cdot}15y+0{\cdot}06=0$ | −1·22 |
| 18 | +0·26 | −1·85 | $x+4{\cdot}35y-0{\cdot}71=0$ | −1·98 |

The normal equations for Phobos are

$$+40{\cdot}25x+6{\cdot}16y-81{\cdot}60=0,$$
$$+6{\cdot}16x+82{\cdot}83y-74{\cdot}28=0;$$

the solution gives

$$x=\Delta u=+1{\cdot}912\pm0{\cdot}3429.$$

The probable error of a single value of $\Delta u$ is $\pm$ 2°·163. We have therefore the value of the periodic time of this satellite

$$T=7^{h}\ 39^{m}\ 13{\cdot}9376^{s}\pm0{\cdot}01382^{s}.$$

The longitude for the epoch Nov. 5.0, 1879, is

$$u=68{\cdot}87\pm0{\cdot}343.$$

The normal equations for Deimos are

$$+46{\cdot}00x+17{\cdot}24y+50{\cdot}45=0,$$
$$+17{\cdot}24x+137{\cdot}20y+24{\cdot}74=0;$$

the solution gives

$$x=\Delta u=-1{\cdot}080\pm0{\cdot}1671,$$

and the probable error for a single value of $\Delta u$ is $\pm$ 1°·106. Hence the periodic time of this satellite is

$$T=30^{h}\ 17^{m}\ 54{\cdot}377^{s}\pm0{\cdot}0949;$$

and the longitude for the epoch is

$$u = 322^{\circ}\cdot 91 \pm 0^{\circ}\cdot 167.$$

After December 1 the weather, which had been unusually favourable, became bad, and the image of the planet was so blazing and unsteady that no further observations of Phobos were made. Although the brighter of the satellites, Phobos is always so near the limb of the planet that it is much more affected by a poor image of Mars. This satellite was seen last on December 16. On December 18, Deimos could be observed with tolerable accuracy, and this satellite was seen last, with certainty, on December 26, but it was too faint to observe. When all the observations of these satellites are published, I hope to undertake their discussion and the determination of new elements.

Although these satellites were brighter than I expected to see them in the Opposition of 1879, still the formula

$$\text{Brightness} = \frac{C}{r^2\Delta^2}$$

will probably represent the brightness for different Oppositions with a fair approximation to the truth. Assuming this brightness to be unity on October 1, 1877, the following table gives the brightness for several dates in 1879 and in 1881 :—

| Date. | Brightness. |
|---|---|
| 1879, Sept. 21 | 0·491 |
| Nov. 5 | 0·730 |
| Dec. 12 | 0·423 |
| 18 | 0·371 |
| 26 | 0·335 |
| 1881, Dec. 9 | 0·389 |
| 31 | 0·384 |

These satellites will therefore be visible for several weeks in December 1881, in the Washington 26-inch Refractor, the declination of the planet being + 26°. Unfortunately this Opposition occurs at a season of the year when the weather is generally very unfavourable for making observations at the northern observatories. If such a powerful telescope as Mr. Commons' reflector could be mounted at Malta or Madeira, perhaps a series of observations of these satellites might be secured in the Opposition of 1881. Such a position would also be favourable for observing the satellites of Saturn, whose oppositions will occur in the autumn, and which is coming into a good position for observations of its Ring and faint satellites.

As the Washington observations of Hyperion, the faint satellite of Saturn, have been referred to, I will say that I observed this satellite on 32 nights in 1878, and on 28 nights in 1879.

The Washington observations of the satellites of Saturn need revision and a complete reduction, and as I have not found time to do this, their publication is delayed; but anyone who needs my observations of Hyperion can have them.

1880, *January* 27.
*Washington, U.S.*

*Observations of Mimas and of an Occultation of Rhea in* 1879 *with the* 26*-inch Equatoreal at Washington.* By Prof. Edward S. Holden, U.S.N.

(*Communicated by Rear-Admiral John Rodgers, U.S.N., Superintendent U.S. Naval Observatory.*)

1879. *Sept.* 29.

W. m. t. $p$ (estimated) 270°. Mag. Power 600 A; Wt. 4.

$10^h\ 27^m$. $p = 270°{\cdot}2$ (4).
$10^h\ 59^m$. $s = 28''{\cdot}62$ (4); satellite very faint; moonlight.

1879. *Sept.* 30.

$p$ (estimated) 285°. Mag. Power 600 A; Wt. 4.

$10^h\ 27^m$. $(s) = 24''{\cdot}14$ (4) distance measured in the line of major axis of the ring; nearly full moon about 10° distant.

1879. *Sept.* 30.
Mag. Power 800 A.

$10^h\ 50^m$. I do not think it is up yet, to *s. p.* the end of the ring, but it is very nearly up.
$10^h\ 54^m$. I suspect I have made a mistake and that it was a little past at $10^h\ 50^m$; it seems to be past now. The seeing is growing quite unsteady (Wt. 2) and *Mimas* is v. F.
$10^h\ 59^m$. *Mimas* not seen after this with eye-pieces 400 A and 600 A.

1879. *Sept.* 30.
Mag. Power 400 A; Wt. 2.

[Occultation (disappearance) of *Rhea*.

$11^h\ 3^m$. *Rhea* is still visible, but very close to the limb of ball.
$11^h\ 10^m$. I no longer see any dark space between the satellite and the ball, but the planet's limb is unsteady.
$11^h\ 13^m$. Satellite seen less than half the time.
$11^h\ 15^m$. Satellite nearly smooth with the limb. Seeing much better (Wt. 4).

W. m. t.

$11^h\ 22^m$. Satellite seen perhaps a third of the time.
$11^h\ 23^m$. Seen for an instant.
$11^h\ 24^m$. " "

*Rhea* not seen after $11\ 24^m$. Very satisfactory observation.

The satellite seemed to grow smaller and more intense white in colour as it came close to the ball of *Saturn*. The colour may have been due to contrast with that portion of the ball near which it impinged. This was the south polar belt, which was a very dark dull olive.

The satellite certainly appeared smaller than usual and smaller than *Dione*, which was near. It is usually larger than the latter satellite.]

1879. *Oct.* 5.

Mag. Power 800 A; Wt. 4.

$9^h\ 41^m$. *Mimas* is just visible approaching *s.f.* conjunction—and very faint.
$10^h\ 11^m$. The sky is hazy, so much so that *Enceladus* near elongation (E) is quite faint, and the larger satellites show that there is a fog or haze around them.

Under these conditions I am not absolutely sure that this is *Mimas*, but if it is, it is just past, or a little past *s.f.* conjunction at $10^h\ 11^m$ W. m. t.

1879. *Oct.* 11.

$12^h\ 20^m$. *Mimas* first seen at this time (on account of clear space through the thin clouds or fog) and it is past the *n. p.* conjunction with end of ring by several degrees.

1879. *Oct.* 12.

Mag. Power 800 A; Wt. 2.

$10^h\ 27^m$. *Mimas* not yet up to *n. p.* Conjunction with end of ring.
$10^h\ 35^m$. A little beyond the principal division. *Mimas* not seen again till $10^h\ 55^m$, and then only for a moment.
$10^h\ 56^m$. It is probably past. Not seen again till
$11^h\ 8^m$. when it was certainly past. Only seen for a moment.
$11^h\ 12^m$. Certainly past (Hall).

Whole observation unsatisfactory, on account of unsteady atmosphere.

1879. *Oct.* 14.

Mag. Power 800 A; Wt. 3. Very hazy.

$8^h\ 27^m$. *Mimas* is, I fear, past [*n.p.*] conjunction a very little. This is the first I have seen of it. The planet is low

W. m. t.

and the sky hazy. *Mimas* cannot be seen with less than 800 mag. power, and 800 makes the images unsteady.

$8^h\ 32^m$. *Mimas* is certainly past, though I can only see it by glimpses. *Enceladus* near elongation is extremely faint.

$9^h\ 13^m$. *Mimas* is just barely seen with 800 A, and not with 600 A.

1879. *Oct.* 16.

$p$ (estimated) 260°. Mag. Power 800 A; Wt. 3.

$10^h\ 27^m.5$. *Mimas* is pretty constantly seen, but is very faint. The sky is hazy.

$10^h\ 51^m.5$. *Mimas* about 4″ *west* of the meridian through the end of the ring.

$11^h\ 4^m.5$. Nearly up.

$11^h\ 13^m.5$. Up.

$11^h\ 17^m.5$. Up, or possibly just past.

*Mimas* not seen after $11^h\ 18^m$, and only seen four times between that and $10^h\ 50^m$. Sky very hazy and observation unsatisfactory in the highest degree.

The above observations were all that could be obtained during the Opposition, using diligence. They are copied literally from the observing books.

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*Observations of Saturn's Satellites, made at Mr. E. Crossley's Observatory, Bermerside, Halifax.* By Mr. J. Gledhill, F.R.A.S.

The instrument with which the following observations were made is an Equatoreal having an aperture of $9\frac{1}{3}$ inches.

Nov. 28th, 1879. Good definition. Power 282. The web was kept bisecting the ball.

*Rhea* in conjunction with the centre of the ball.

| h. | m. | |
|---|---|---|
| 10 | 40 G.M.T. | Conjunction has not yet occurred. |
| | 48 | In conjunction. |
| 11 | 0 | Certainly past conjunction. |

Clouds prevented the observation of Tethys and Enceladus.

Nov. 29th, 1879. Good definition. Power 282. The web was set perpendicular to the plane of the ring.

*Tethys* in conjunction with west end of ring: *np*.

| | h. | m. | |
|---|---|---|---|
| (*a*) | 7 | 20 | Not yet in conjunction. |
| (*b*) | | 25 | In conjunction. |
| (*c*) | | 30 | Certainly just past. |

(*a*), (*c*) good; (*b*) probably the best observation.

Dec. 6th. *Dione* in conjunction with end of ring: *nf*.

| h. | m. | |
|---|---|---|
| 9 | 30 | Not up yet. |
| | 35 | In conjunction. |
| | 40 | Past. |

Power 282. Pretty good definition.

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*Notes on the Physical Appearance of Jupiter: being part of the Paper "Observations of Eclipses, Occultations and Phenomena of Jupiter's Satellites made with the 8-inch Equatoreal (Cooke) at the Adelaide Observatory, South Australia" (ante, pp. 170–176).* By C. Todd, Esq., Director of the Observatory.

1878, July 5, 10.25 P.M.—North of the equatoreal belt was a well-defined streak of dark salmon colour; the dark belt on the south of the equator was covered with scattered white-looking clouds, to the south of which was a bright belt, having a remarkable bright spot, like a cumulus cloud, on the southern edge; on the meridian, to the east of which, at some distance, was a well-defined rectangular indentation.—(T.)

July 21, 9.45 P.M.—The appearance of the salmon-coloured streak was as if viewed through a thin veil of cloud. On the south edge of the equatoreal cloud belt was an oval brilliantly white cloud, nearly on the equator and exactly mid planet.—(T.)

July 26, 8.35 P.M.—At three minutes after observation No. 23, the shadow was about its own diameter inside the limb of planet, and immediately following the satellite—in fact, touching it to all appearance. The satellite entered on the dark band north of the equator, the central portions of which were of a bright sienna, or a nearly rose colour. The equatoreal cloud belt was generally diffused, but its central parts were very dense, having the appearance and shape of cumulus clouds. The dark belts have still the appearance as if seen through hazy cloud or fog. The satellite when it first entered planet appeared large and bright, but became gradually smaller—still bright—and the shadow seemed larger than the satellite.—(R.)

July 29, 8.35 P.M.—The equatoreal belt is not so broad or so well defined as usual, and consists of comparatively narrow streaks of broken cloud on the southern edge; the northern dark belt is not at all well marked, and is of a rose salmon colour, especially on the north side; the southern dark belt is broken by streaks of white cloud, and is not so well marked or defined, although broader than the northern belt, and is less highly coloured.—(T.)

July 29, 10.50 P.M.—No. 1 satellite and its shadow traversing the north portion of the equatoreal belt, and almost touching each other; the shadow

a little to the north of satellite, the south pole of the shadow being in line with the equator of the satellite. The equatoreal belt is broader than before, and the north brown belt darker; the southern dark belt is broken by a white streak, stretching nearly across the planet, and is hazy, and not so red or brown as the north belt.—(T.)

August 9, 7.50 P.M.—*Jupiter* well defined. The long, oval, dark reddish patch south of the southern belt, noticed on the 4th, still visible, but I think slightly longer. The bright patch on bright belt not distinguishable.—(T.)

August 13, 10.0 P.M.—The oval red space on southern bright belt, noted on the 4th, still visible, but not the bright spot on the equatoreal cloud belt, but a bright spot very like it precedes it several Jovian hours, being near the western edge of the planet when the red space was rather more than half visible on the opposite or eastern side of the planet.—(T.)

October 1, 10.0 P.M.—The equatoreal cloud belt was dense, and the occultation was complete at limb. It may be here remarked, as a general note, that the satellites, as a rule, are not visible through the dense white cloud belts, but are seen at occultation through the broken cloud belts and brown belts.—(T.)

October 2, 10.0 P.M.—The southern dark belt this year has generally been covered with a thin film of white cloud, and has seldom the decided salmon tint noticed in previous years, occasionally assuming a greyish green. The equatoreal bright belt to-night resembled a bank of cumulus cloud with its base toward the north and its crest toward the south. The northern dark belt is narrow, and of a salmon tint, varied by dark streaks and patches.—(T.)

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*Phenomena of Jupiter's Satellites observed at Mr. E. Crossley's Observatory, Bermerside, Halifax.* By Mr. J. Gledhill, F.R.A.S.

The instrument with which the following observations were made is an Equatoreal having an aperture of $9\frac{1}{3}$ inches. The power used was almost invariably 240: occasionally powers 330 and 470 were applied:—

| Day of Obsn. 1879 | Satellite | Phenomenon | G.M.T. of Obsn. h. m. s. | Time from N.A. h. m. s. | Remarks |
|---|---|---|---|---|---|
| Aug. 30 | I. | Oc. R. first seen | 10 6 | 10 8 | very bad definition |
| | | half out | 7 | | |
| | | just off | 8 30 | | |
| 31 | III. | Tr. E. first contact | 11 11 | 11 19 | very bad definition |
| | | half off | 15 | | |
| | | just off | 19 | | |
| Sept. 14 | I. | Tr. I. first contact | 8 29 | 8 29 | bad definition |
| | | bisection | 30 30 | | |
| | | second contact | 32 | | |
| 14 | I. | Sh. I. bisected | 8 52 30 | 8 51 | bad definition |
| | | fully on disk | 55 | | |
| 14 | II. | Oc. D. first contact | 10 14 | 10 15 | bad definition |
| | | disappearance | 18 | | |

| Day. of Obsn. 1879 | Satellite | Phenomenon | G.M.T. of Obsn. h. m. s. | Time from N.A. h. m. s. | Remarks |
|---|---|---|---|---|---|
| Sept. 29 | I. | Oc. D. first contact | 9 13 | | very fair obsn. |
| | | bisection | 15 | 9 15 | very fair obsn. |
| | | disappearance | 17 | | very good |
| 29 | I. | Ec. R. first seen | 12 15 50 | 12 15 8 | fair |
| Oct. 6 | I. | Oc. D. first contact | 11 1 | 11 2 | |
| | | disappearance | 11 3 | | |
| 9 | II. | Oc. D. disappeared | 6 5 | 6 15 | bad definition |
| 14 | I. | Tr. I. first contact | 9 59 | | fair observation |
| | | bisection | 10 1 | 9 59 | fair observation |
| | | second contact | 10 2 | | fair observation |
| 14 | I. | Sh. I. fully on disk | 11 3 | 10 59 | very bad defn. |
| 15 | I. | Ec. R. first seen | 10 34 43 | 10 35 8 | good obsn. |
| | | fully out | 36 | | uncertain |
| 20 | III. | Tr. I. first contact | 7 32 48 | 7 32 | much boiling |
| | | bisection | 36 | | |
| | III. | Tr. E. inner contact | 11 0 | | fair observation |
| | | bisection | 3 | 11 7 | uncertain |
| | | external contact | 8 | | fair |
| 25 | II. | Tr. I. just on disk | 6 24 | 6 3 | very bad defn. |
| 25 | II. | Sh. I. first seen | 8 23 30 | | |
| | | half on | 24 | 8 22 | |
| | | fully on | 25 | | good obsn. |
| 25 | II. | Tr. E. inner contact | 8 48 30 | | fair observation |
| | | bisection | 50 30 | 8 55 | fair observation |
| | | external contact | 53 | | fair observation |
| 25 | IV. | Ec. D. began to fade | 9 25 | 9 34 29 | misty, but still |
| | | gone | 27 | | |
| 25 | II. | Sh. E. off disk | 11 7 | 11 14 | |
| Nov. 1 | I. | Sh. E. just off | 6 7 | 6 6 | very bad defn. |
| 1 | II. | Tr. I. just within | 8 33 | 8 32 | very bad defn. |
| 1 | II. | Sh. I. just within | 11 3 | 11 1 | very bad defn. |
| 1 | II. | Tr. E. just off | 11 25 | 11 24 | very bad defn. |
| 2 | IV. | Tr. I. certainly on disk | 8 20 | 8 30 | |
| 7 | I. | Oc. D. first contact | | | fair observation |
| | | bisection | 15 30 | 7 17 | fair observation |
| | | just gone | 17 | | good obsn. |
| 12 | II. | Sh. E. first contact | 5 35 | 5 48 | very bad defn. |

| Day of Obsn. 1879 | Satellite | Phenomenon | G.M.T. of Obsn. h. m. s. | Time from N.A. h. m. s. | Remarks |
|---|---|---|---|---|---|
| Nov. 14 | III. | Oc. D. first contact | 8 32 | | |
| | | bisection | 35 | 8 38 | bad definition |
| | | just gone | 39 | | |
| 14 | I. | Oc. D. first contact | 9 8 | | good obsn. |
| | | bisection | 10 | 9 11 | not good |
| | | just gone | 11 | | good |
| 15 | I. | Tr. I. first contact | 6 19 | | good obsn. |
| | | bisection | 21 | 6 19 | good? |
| | | just within | 22 | | good |
| 15 | I. | Sh. I. just within | 7 40 | 7 39 | |
| 15 | I. | Tr. E. inner contact | 8 36 | | uncertain obsn. |
| | | bisection | 38 | 8 38 | good |
| | | outer contact | 40 | | good |
| 23 | I. | Ec. R. first seen | 9 9 53 | | good obsn. |
| * | | full orb | 11 30 | 9 11 19 | good obsn. |
| 24 | I. | Tr. E. outer contact | 5 0 | 5 1 | cloudy |
| Dec. 3 | II. | Tr. I. first contact | 8 10 | | |
| | | bisection | 12 | 8 8 | pretty good definition |
| | | just within | 14 | | |
| 6 | III. | Ec. R. first seen | 5 28 | | |
| | | full orb | 31 | 5 30 42 | bad definition |
| 6 | IV. | Sh. I. just within | 8 58 | 8 56 | very bad sky |
| 8 | I. | Tr. I. just within | 6 38 | 6 35 | good defn. |
| 8 | I. | Sh. I. just within | 7 56 | 7 55 | |
| 8 | I. | Tr. E. inner contact | 8 51 | | |
| | | external contact | 56 | 8 54 | pretty good sky |
| 12 | II. | Oc. D. first contact | 4 58 30 | | |
| | | half gone | 5 0 30 | 4 59 | good sky |
| | | just gone | 2 30 | | |
| 21 | II. | Sh. I. just within | 5 29 | 5 25 | very bad defn. |
| 21 | II. | Tr. E. inner contact | 5 45 | | |
| | | bisection | 47 | 5 46 | |
| | | outer contact | 50 | | |

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* See foot-note, p. 291.

A few phenomena observed with the same instrument in the years 1875, 1876, and 1877 are subjoined.

| Day of Obsn. | Satellite | Phenomenon | G.M.T. of Obsn. | Time from N.A. | Remarks |
|---|---|---|---|---|---|
| 1875 | | | h. m. s. | h. m. s. | |
| April 13 | I. | Ec. D. began to fade | 9 17 53 | | good |
| * | | half gone | 18 18 | 9 19 11·6 | not so good |
| | | disappeared | 19 18 | | good |
| | I. | Oc. R. first seen | 11 29 30 | | bad definition |
| | | half out | 31 | 11 34 | bad definition |
| | | just clear | 33 | | bad definition |
| 19 | I. | Tr. I. first contact | 13 46 30 | | not good |
| | | bisection | 48 | 13 49 | not good |
| | | just within | 49 30 | | pretty fair |
| 19 | I. | Sh. I. fully on disk | 13 54 | 13 52 | uncertain |
| 28 | I. | Tr. I. first contact | 9 57 57 | | uncertain |
| | | bisection | 59 27 | 9 59 | uncertain |
| | | just within | 10 1 27 | | uncertain |
| 28 | I. | Sh. I. bisected | 10 16 | 10 15 | defn. very bad |
| 28 | I. | Tr. E. first contact | 12 6 27 | | definition bad |
| | | bisection | 8 27 | 12 11 | |
| | | just clear | 11 27 | | the best obsn. |
| May 14 | I. | Tr. E. first contact | 10 4 4 | | good |
| | | bisection | 5 4 | 10 8 | fair |
| | | just off | 7 34 | | good |
| | I. | Sh. E. first contact | 10 42 34 | | fair |
| | | bisection | 44 34 | 10 46 | fair |
| | | last contact | 46 34 | | fair |
| 22 | I. | Ec. R. first seen | 9 53 49 | | good |
| | | half out | 54 28 | 9 54 3·6 | uncertain |
| | | full orb | 55 40 | | good |
| June 13 | I. | Tr. I. first contact | 9 33 28 | | pretty good |
| | | bisection | 35 58 | 9 35 | uncertain |
| | | last contact | 37 58 | | pretty good |
| 13 | I. | Sh. I. bisection | 10 40 | 10 42 | bad sky |
| | | just within | 43 | | bad sky |
| 14 | I. | Ec. R. first seen | 10 5 39 | 10 5 48·5 | good |
| 14 | II. | Ec. R. first seen | 10 49 14 | 10 52 0·6 | good |
| | | full orb | 52 | | good |

| Day of Obsn. | Satellite | Phenomenon | G.M.T. of Obsn. | Time from N.A. | Remarks |
|---|---|---|---|---|---|
| 1876 | | | h. m. s. | h. m. s. | |
| April 7 | I. | Sh. I. inner contact | 12 57 | 12 56 | bad definition |
| 7 | I. | Tr. I. inner contact | 13 49 | 13 48 | bad definition |
| 7 | I. | Sh. E. just off | 15 9 | 15 8 | bad definition |
| 7 | I. | Tr. E. just off | 16 0 | 15 59 | bad definition |
| 15 | I. | Ec. D. just gone | 12 12 | 12 19 1·5 | not good |
| 26 | III. | Ec. D. just gone | 11 50 | 11 42 3.6 | |
| 26 | III. | Ec. R. first seen | 13 24 | 13 23 45·9 | probably some seconds late |
| 26 | III. | Oc. D. just gone | 13 50 | 13 48 | |
| 30 | I. | Sh. I. inner contact | 13 7 | 13 5 | bad definition |
| 30 | I. | Tr. I. inner contact | 13 31 | 13 30 | bad definition |
| 30 | I. | Sh. E. just off | 15 19 | 15 17 | bad definition |
| May 2 | I. | Sh. E. just off | 9 48 | 9 46 | fair definition |
| 2 | I. | Tr. E. just off | 10 9 | 10 7 | |
| 7 | I. | Sh. I. just within | 15 0 | 14 59 | very bad sky |
| 7 | I. | Tr. I. just within | 15 15 | 15 14 | very bad sky |
| 8 | I. | Ec. D. first seen | 12 23 | 12 21 55·9 | very bad sky |
| 9 | I. | Sh. I. inner contact | 9 29 | 9 27 | very bad sky |
| 9 | I. | Tr. I. inner contact | 9 41 | 9 39 | very bad sky |
| 9 | I. | Sh. E. just off | 11 42 | 11 40 | very bad sky |
| 9 | I. | Tr. E. just off | 11 53 | 11 51 | very bad sky |
| June 1 | I. | Sh. I. inner contact | 9 40 | 9 39 | bad definition |
| 1 | I. | Tr. E. inner contact | 11 26 | 11 29 | bad definition |
| | | outer contact | 29 | | bad definition |
| 1 | I. | Sh. E. just off | 11 51 | 11 51 | bad definition |
| 16 | I. | Oc. D. first contact | 10 4 | 10 7 | uncertain |
| | | gone | 8 46 | | pretty good |
| July 2 | I. | Ec. R. first seen | 11 15 13 | 11 15 17·7 | good |
| | | full orb | 7 | | |

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| | | | | | |
|---|---|---|---|---|---|
| 1877 | | | | | |
| July 1 | II. | Tr. I. first contact | 10 12 | 10 14 | bad definition |
| * | | bisection | 14 | | |
| | | inner contact | 16 | | |
| 26 | II. | Tr. E. just off | 9 1 | 9 3 | very bad sky |

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* During the observations marked thus * Mr. Crossley was at the telescope and I watched the chronometer.

| Day of Obsn. | Satellite | Phenomenon | G.M.T. of Obsn. | Time from N.A. | Remarks |
|---|---|---|---|---|---|
| 1878 | | | h. m. s. | h. m. s. | |
| Sept. 10 | III. | Oc. D. first contact | 9 0 | | a little uncertain |
| * | | half gone | 4 | 9 4 | very fair |
| | | gone | 8 | | good |
| | I. | Ec. R. first seen | 10 39 1 | | good |
| | | half out | 39 49 | 10 38 33 | uncertain |
| * | | fully out | 40 49 | | uncertain |
| Oct. 26 | I. | Oc. D. first contact | 7 29 | 7 31 | bad sky |

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*Observations of the exterior Satellite of Mars at the Oxford University Observatory.* By Prof. C Pritchard.

The accompanying observations of the exterior satellite of Mars were made with the Grubb Equatoreal of 12¼ inches' aperture, the power employed 125. During the observations the planet was nearly hidden behind a bar which could be turned with the Position Circle. As no illumination of the field could be permitted, the position angles were determined by turning the bar till it was at right angles to the line joining the satellite and the centre of the planet, the judgment being assisted by the small segment of the planet left uncovered. It was thought that this angle could be judged with some degree of accuracy. The distances are estimations, assisted by a knowledge of the width of the bar and of the diameter of the planet.

| Oxford Sidereal Time. | Position Angle | Distance. | Remarks. |
|---|---|---|---|
| h m | ° | ″ | |
| Nov. 5 0 57·2 | 58 | 62 | The satellite kept pretty steadily in view. The error of the position angle probably within 5°. Definition very favourable. Some moonlight. |
| 1 0·4 | 55 | 60 | |
| 1 45·5 | 55 | 65 | |
| Nov. 11 22 27 | 223 | 55 | False scattered light beyond the limb of the planet very troublesome. The definition of the planet not so good as on the 5th inst., and the observation less satisfactory. |
| 23 0 | 225 | 50 | |
| 23 15 | 225 | 48 | |
| Nov. 15 3 29 | 60 | 60 | It was thought at times that there was an object on the other side of the planet at about half of this distance. |
| 4 58 | 57 | 62 | |

These observations were made by Mr. W. Plummer.

*Oxford,*
1880, *March* 11.

## *Note on Mr. Maxwell Hall's Paper on the Nebula in the Pleiades.*

By Dr. C. Wolf.

Je trouve dans le dernier No. des *Monthly Notices*, qui vient de m'arriver, une note de M. Maxwell Hall sur la nébuleuse des Pléiades, et la requête qu'il adresse en terminant aux astronomes de vouloir bien s'occuper de nouveau de cet intéressant objet. Je crois que la réponse complète à la demande de M. Hall est donnée dans le mémoire, 'Description du Groupe des Pléiades,' que j'ai publié en 1877 dans les *Annales de l'Observatoire de Paris*, et dont j'ai offert un exemplaire à la Société Royale Astronomique.

J'ai résumé dans ce mémoire l'historique complet de la nébuleuse en question, et montré pourquoi les observateurs ont tant différé d'opinion sur sa forme et sur sa visibilité. Comme le montre ma carte des Pléiades, la Nébuleuse, telle que Goldschmidt l'a vue le premier dès 1864, *entoure complètement* le groupe de ces étoiles, qui brillent sur le fond noir du ciel, comme dans un trou percé au milieu de la nébulosité. Elle est médiocrement brillante, et l'absence de contours bien tranchés, sauf dans la région de Mérope, la rend difficile à voir même par un ciel très pur. Le moyen qui m'a le mieux réussi consiste à regarder les fils d'araignée du réticule, en même temps qu'on déplace peu à peu la lunette. Sur le fond noir du ciel, au milieu du groupe des Pléiades, les fils disparaissent complètement; ils se détachent en noir dès qu'une portion du champ est envahie par la nébulosité. On comprend donc comment, avec un puissant instrument dont le champ est toujours très restreint, cette nébulosité échappe aisément à la vue. La plus légère brume suffit d'ailleurs à égaliser l'éclairement du fond du ciel et à faire disparaître toute trace de la nébuleuse, lors même qu'elle ne voile pas les plus faibles étoiles de 13me et de 14me grandeur. Toutes mes observations ont été faites avec un objectif de 33 centimètres d'ouverture.

Les diverses portions de la nébuleuse sont très inégalement brillantes. La plus facile à voir est le grand éventail dont Mérope occupe le sommet: c'est la région vue par Tempel et la plupart des observateurs. Elle se prolonge vers Electre et au-delà de Cœleno, comme l'a dit Schiaparelli. Dès 1768, Teaurat paraît en avoir aperçu un autre lambeau, qui forme un second maximum auprès des deux anonymes 31 et 32 de Bessel.

Les différences d'aspect de cette nébuleuse suivant l'état du ciel et l'étendue du champ de la lunette employée, ont fait penser qu'elle pouvait être variable. J'avais moi-même à l'origine énoncé cette opinion. Il est aujourd'hui certain pour moi qu'elle n'a pas varié depuis 1864.

*Paris*,
1880, 11 *Janvier*.

## *Note on the Spectrum of Mr. Baxendell's new Star in Canis Minor.* By Dr. H. C. Vogel.

(*Extract from a letter addressed to the Foreign Secretary.*)

I permit myself to communicate to you my observations concerning the new star found by Mr. Baxendell.

The new star was observed by Dr. Müller and me on Dec. 16, 1879, and on Feb. 7, 1880. The colour is reddish yellow, magnitude on Feb. $7 = 9^{m}\cdot3$. The spectrum is highly interesting. The blue and violet are strongly absorbed, so that at the first glance only half the spectrum seems to exist. In the blue and violet three dark, broad bands are plainly recognised. In the green and red there appear also to be bands, but they are too faint to be certainly distinguished. On Feb. 7 I examined also the spectrum of the preceding star B.D.+8°, 1846, which has a yellow colour, and found that this star has a similar spectrum to that of the new star, except that it is not so clearly developed.

The star B.D. + 8°, 1848, gives a continuous spectrum without bars or bands.

I am under the necessity of supposing that in the spectroscopical observation Mr. Lohse has made a mistake, and in place of the new star has observed No. 1848.

*Astrophysical Observatory, Potsdam,*
1880, *Feb.* 10.

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## *Supplementary Note to the Paper of Dr. Vogel.* By Lord Lindsay.

Baxendell's Nova has been repeatedly observed at Dunecht with the spectroscope. It was not considered an interesting object.

Herr G. L. Lohse measured the limits of the spectrum on March 7, to be—

| | Spectrum begins | Spectrum ends |
|---|---|---|
| Nova | $647^{mmm}$ | $456^{mmm}$ |

with a narrow slit. Again, with a wide slit, on March 9,

| | | |
|---|---|---|
| Nova | 660 | 432 |

In the blue, near F, is one broad darkish band.

On March 9 the magnitude was estimated at M. 9·5, certainly not brighter.

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## *Observations of the Great Southern Comet* 1880, I. *made at Monte Video.* By Lieut. B. Gwynne.

(*Communicated by the Secretaries.*)

The accompanying diagrams* are intended to show the appearance and relative position of the tail of a large comet with reference to some fixed stars near which it was situated, observed at Monte Video (Rio de la Plata) on the 1st, 2nd, 3rd, 4th and 8th February, the overcast state of the sky preventing my observing its position on the intermediate evenings (4th to 8th).

On the 1st February, after a long twilight, the comet's tail was observed in a S.S.W. direction, having an altitude of about 20° above the horizon.

Subsequent observations show the comet to have an apparent motion of 2° to the westward in twenty-four hours.

The brilliancy of the comet's tail was, on the 1st, about that of the Milky Way on a clear frosty night, and each succeeding day showed a diminution in brilliancy until the 8th, when it was difficult to trace the direction of the tail, although the night was favourably dark.

No vestige of a nucleus could be detected with a ship's spy-glass (sufficiently powerful to see *Jupiter's* satellites with).

Therefore I am unable, for want of space and time, to give a clearer idea of the comet's position and apparent motion than with a few diagrams.

I am of opinion that the comet was in or near perihelion on the 1st of February and that the tail preceded the nucleus in its motion through space. Doubtless, observations of value have been made at the Cape, Australia and Cordoba (South America), and other Observatories.

*H.M.S. "Garnet,' Monte Video,*
*1880, February 9.*

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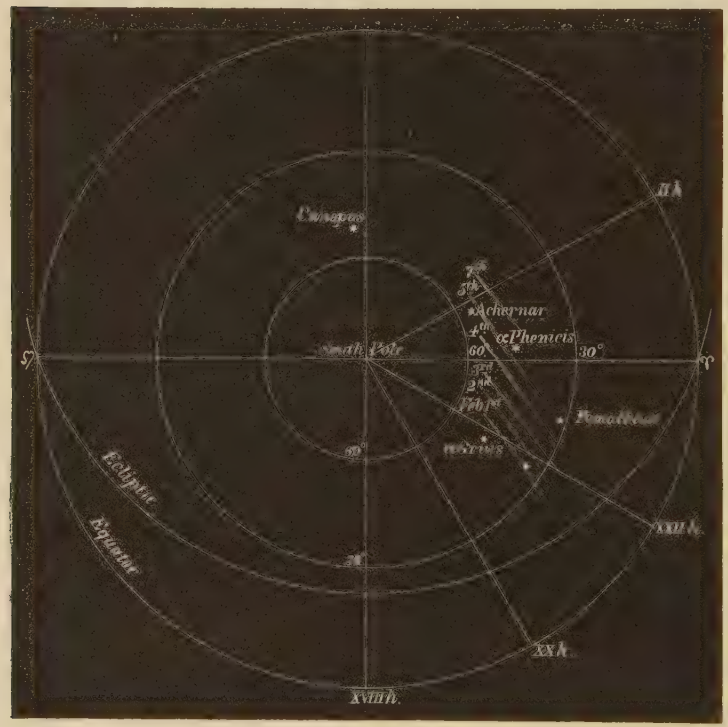
## *Observations of the Great Southern Comet* 1880, I. *from Feb. 1 to Feb. 7, made at Monte Video.* By the Rev. S. S. O. Morris.

(*Communicated by the Astronomer Royal.*)

The tail of a comet was observed from H.M.S. 'Garnet,' at about 8 P.M. on the evening of Sunday the 1st Feb., 1880. The 'Garnet' was at the time lying in the outer roads at Monte Video. The nucleus of the comet was at the time beneath the horizon. The visible part of the tail cut the horizon at about the S.S.W. point nearly at right angles, and the altitude of the extremity of the tail visible to the naked eye was about 30°. The tail lay

* The diagrams were exhibited at the Meeting.—Ed.

across the constellations of *Grus* and *Piscis Australis* in direction parallel to the line of direction of the Milky Way. The R.A. of the nucleus conjectured from the direction of the tail and the position of the Sun was at 8 P.M. on the 1st Feb. about 21h. Two small stars of *Piscis Australis* were visible through the tail. These two small stars lying close together were noted as a mark whereby the motion of the comet could be observed on future nights. There was a slight arch in the tail concave towards the south pole, convex towards Fomalhaut, which lay to the west of




the comet. The tail was again observed on the evening of the 2nd; the nucleus was not visible. The position of the tail at 8 P.M. was altered from the previous evening; it had moved from the two bright small stars in *Pisc. Austr.* and had approached Fomalhaut; at the same time its extremity had a greater altitude than on the night of the 1st Feb. We watched carefully for the first appearance of the comet on the evening of the 3rd, but, although the tail, now of immense dimensions, soon became visible, the nucleus had not yet become visible. The position of the tail on the night of the 3rd was to the right and west of its

position on the night of the 2nd. The successive positions of the tail on the nights of the 1st, 2nd, and 3rd implied a motion in R.A. of the nucleus of about $10^m$ in 24 hours. The increase in South Declination of the nucleus would be very roughly about 4°. On this night the end of the tail was not far off Achernar (*α Eridani*). On the 4th the nucleus was not yet visible; the position of the tail on this night established the general path in the heavens of the nucleus—viz. about $10^m$ increase in R.A., 4° increase in S. Decl. An arc of a great circle passing through Canopus and Achernar would have very nearly coincided with the axis of the tail. The evening of the 5th was overcast, but the clouds broke sufficiently for it to be seen that the motion already conjectured had continued. The evening of the 6th was overcast. The evening of the 7th was cloudy, but in a break of the clouds the comet's tail was seen to the right of Achernar and now extending much beyond Achernar.

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*Note on the Great Southern Comet* 1880, I. *as seen at Melbourne, Feb. 2 to Feb. 5.* By R. L. J. Ellery, Esq.

A large comet, low on the western horizon, was first seen here on the evening of February 2. Owing to its lowness and the fact of the horizon being very hazy every evening since its appearance no observation of the nucleus has yet been possible: indeed, the nucleus has not been seen at this Observatory at all, although it is stated to have been seen in other parts of the colony.

On Feb. 2 the tail was seen extending above the horizon reaching to *β Gruis*; it tended towards west, making an angle of 60° with the horizon. It has been observed every night since, and last night the tail reached a little beyond *ε Phœnicis*—the north edge passing through it. It is much less bright than on Feb. 2, but much longer; and on Feb. 4 the tail could occasionally be traced nearly to the horizon without any appearance of condensation, while it extended upwards to the prolongation of the line joining *α* and *ε Phœnicis*.

Probably other observers in Australia have been more fortunate than we have, but this brief account from here may not be without interest.

The weather has been unusually clear for some weeks past and the horizon was carefully scanned on the evening of Feb. 1, but nothing was seen of the comet till the evening of the 2nd.

*Observatory, Melbourne,*
1880, *February* 6.

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*Observations of the Great Southern Comet* 1880, I. *made at the Adelaide Observatory, New South Wales.* By Charles Todd, Esq., C.M.G.

(*Extract from a letter to the Astronomer Royal.*)

I just write a few lines to advise you of a fine comet that has unexpectedly come into view during the past week. I have not yet succeeded in seeing the nucleus, which is probably below the horizon, but the tail is of great length and is a most conspicuous object in the south west after dark. It appears to have been seen on Sunday evening, February 1. I first saw it on Monday evening between $8^h$ and $9^h$, when in St. Vincent's Gulf on my way from the fine steamer *Orient*. It then appeared as a narrow whitish auroral streak extending from the horizon (North of S.W.) and passing nearly through the stars $\delta^1$, $\delta^2$, and $\beta$ *Gruis*, the upper portion curving somewhat sharply to the south. Since then the tail has increased in altitude and swept over the sky in a northerly direction, as though turning about a centre a little below the horizon, the upper extremity having a more rapid motion than that near the horizon. The following notes will sufficiently indicate the position of the tail on each night since it was first seen.

February 2nd, $8^h\ 40^m$.—The upper part of tail passes through $\delta^1$, $\delta^2$, and $\beta$ *Gruis*, curving towards $\varepsilon$ *Gruis*. Could not trace it to horizon, but about S.W. by W.

February 3.—Sky rather cloudy in S.W., and view of comet interrupted, but first seen in strong twilight about $8^h$; very bright later, when quite dark. The tail has moved to the north, the stars $\theta$ and $\iota$ *Gruis* lying nearly on the northern edge. Tail curved to southward, and approximately concentric with a line connecting $\delta^1$, $\delta^2$, and $\beta$ *Gruis*, and near the horizon it must pass close to $\beta$ *Piscis Australis*, which, however, is behind a low bank of cloud. Tail brighter to-night, and at $8^h\ 20^m$ could be traced to an altitude of about 27°.

February 4.—Could see the comet at $8^h$; very bright later, when quite dark; certainly brighter than last night, being broadest and most luminous immediately below $\theta$ *Phœnicis*. Could not trace it to horizon; sky thick; $\beta$ *Piscis Australis* at $8^h\ 20^m$ within the tail not far from its northern edge; $\theta$ *Phœnicis* within and close to northern edge, and the tail could be traced nearly up to and pointed almost directly towards $\eta$ *Phœnicis*; could see nothing of nucleus, though I looked carefully for it from soon after sunset directly in course of tail.

February 5.—Tail not so bright, first seen at $8^h\ 10^m$. It reached up to $\zeta$ *Phœnicis* and passed through or close by $\varepsilon$ *Phœnicis*, less curved.

February 6.—The tail certainly fainter and less curved as the extremity sweeps over to the north. It now reaches up to $\chi$ *Eridani*; air thick on horizon.

February 7, $9^h$ P.M.—The tail is much fainter and is reduced to a thin nebulous streak extending from Fomalhaut (over which it passes) up to a little north of $\gamma$ *Phœnicis*, passing directly over $\alpha$ *Phœnicis*; sky thick below Fomalhaut.

We have had a great many meteors—one extraordinary one on Monday morning, February 2, about $10^h$ A.M., which was seen at different places over 200 miles apart. One correspondent describes it as a large ball of fire, as large as the sun, which descended towards the S.W. from an altitude of 60°.

*Observatory, Adelaide,*
1880, *February* 7.

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## *Mr. L. A. Eddie's Observations of the Great Southern Comet 1880, I. at Graham's Town.*

The following is an account of Mr. L. A. Eddie's observations at Graham's Town, derived partly from a letter to the Astronomer Royal and partly from particulars published in the *Eastern Star* newspaper:—

Mr. Eddie first saw the tail on Feb. 2, when it was about 1° in breadth, stretching upwards from the constellation *Piscis Australis* to $\beta$ *Gruis*, 20° above the horizon. It had a decided curvature concave to the south, and shone with a light of a pale straw colour about equal in brightness to that of the Milky Way, and far more brilliant than on any subsequent evening.

Feb. 3 was cloudy and on Feb. 4 the comet's tail was found to have moved eastward about 20°. It was now 40° in length, passing over $\beta$ *Piscis Australis*, and $\theta$ *Phœnicis*, and terminating near a small star about 5° below $\zeta$ *Phœnicis*. It had completely lost its curvature. On account of cloud low down, Mr. Eddie was unable to ascertain whether the nucleus was above the horizon.

On Feb. 5, the sky being very clear, Mr. Eddie discovered about $8^h$ p.m. with a 3-inch refractor, using a power of 80, a faint nucleus about equal in size to the annular nebula in *Lyra*, and resembling 47 *Toucani*, Lacaille's globular cluster, when viewed with a power too low to resolve it. It was of a pale yellow colour, condensed at the centre, and situated about 3° *sp* from Fomalhaut. The tail proceeded from an arc of the nucleus equal to a sixth of the circumference, and spread out as a fan till it reached a breadth of about 1° at a distance of about 20° from the nucleus, and preserved this breadth with but a small, if any, increase to about 50° from the nucleus, where it faded away from sight. The outline throughout the whole length was tolerably well defined, and to the naked eye that part of the tail appeared brightest which lay between 5° and 10° from the nucleus. The tail passed over $\gamma$ *Piscis Australis*, $\varepsilon$ and $\lambda$ *Phœnicis*, and terminated at $\rho$ *Phœnicis*, though by a side glance it could be traced as far as $q^1$ and $q^2$ *Eridani*.

On Feb. 6 clouds hid the nucleus. The tail passed over $\beta$ *Sculptoris*, $\kappa$, $\mu$, $\beta$ and $\delta$ *Phœnicis* and $\chi$ *Eridani*, terminating near $\phi$ *Eridani*.

Feb. 7.—Nucleus so close to $\gamma$ *Sculptoris* that to the naked eye it appeared as if this small star constituted the nucleus. In the 3-inch refractor, with a power of 50 or 60, the nucleus at $8^h\ 30^m$ was *nf* the star at a distance of a little less than half the field. At $9^h\ 15^m$ the nucleus was one-third of the field from the star, and was observed till $9^h\ 20^m$ when it was apparent that it would not transit over $\gamma$ *Sculptoris* but would pass a little to the east. The coma was not distinguishable from the nucleus, but was condensed at the centre. The tail extended from $\gamma$ *Sculptoris* nearly to $\kappa$ *Eridani*, passing north of $\alpha$ *Phœnicis*, over $\nu$ and another small star near $\chi$ *Phœnicis*.

Feb. 8.—The comet, on account of increasing faintness, was not visible till $7^h\ 50^m$, and the nucleus was only found, after much search, at $8^h\ 40^m$, about 5° E. by N. of $\gamma$ *Sculptoris*, in R.A. $23^h\ 30^m$, Dec. 35° S. approximately. The difficulty in finding it arose from the circumstance that the tail appeared to start from a point a little above the nucleus, the faint light of the comet being obscured near the horizon. The tail passed over $\varepsilon$ and $\gamma$ *Phœnicis*, and, passing a little to the right of $\kappa$ *Eridani*, extended right across the constellation *Microscopium*. The light had now become so faint that the comet might easily escape observation.

Feb. 9.—Cloudy. Comet not seen.

Feb. 10.—Evening still cloudy. Comet visible for short intervals. The nucleus was seen in the telescope almost in the same field with $\theta$ *Sculptoris*. The tail passed over $\iota$, $\theta$ and $\varepsilon$ *Eridani*, and across the constellation *Horologium*.

Feb. 11.—Cloudy. Comet not seen.

Feb. 12.—Cloudy. A slight hazy light to be seen now and then through openings in the clouds.

The tail after the first evening was perfectly straight and colourless.

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*Observations of the Great Southern Comet* 1880, I. *made at the Cape of Good Hope, Feb. 2 to Feb. 15.* By David Gill, Esq.

(*Extracts from letters to the Astronomer Royal.*)

By last mail I wrote to tell you that we have a comet by the tail, and I am sorry to say that we only have him by the tail still.

To get any observations at all it was necessary to go to Sea Point, to command the sea horizon to the S.W. of Table Mountain. I selected Mr. Henry Solomon's Garden as the best site, and the site where Maclear observed Donati's Comet in 1858.

I took the Dollond 10-inch altazimuth and a couple of chronometers there on the evening of Tuesday the 3rd inst., but the horizon was somewhat hazy and $\beta$ *Piscis Australis* could hardly be made out, and the tail could not be traced so far.

(To face p. 300.)

MR GILL — THE GREAT SOUTHERN COMET.

Spottiswoode & Co Lith London

February 4 was a very fine night, except for a low bank of cloud on horizon. I could not make quite certain as to how the comet's tail passed *β Piscis Australis*. The rest of the drawing is very accurate.

February 5 and 6.—Very fine. On the latter day the tail had a curious curve. Drawings very accurate, except where outline is dotted.

February 7.—Less fine, and comet fainter. Some uncertainty about limit of comet. I certainly saw no nucleus, but others up country say it was seen.

February 8 was a cloudy night. In a break of cloud I picked up with an opera-glass a nebulous nucleus, with no well-defined point to the south of *μ Sculptoris*; but before I could bring the altazimuth to bear, the cloud had formed over the spot.

February 9.—From the faintness of the nucleus it seemed quite useless to attempt further observations with a portable telescope, so I remained here last night. We had only a glimpse of the nucleus with an opera-glass for a few seconds, and afterwards saw the tail portion by portion; but even before the nucleus could be got into the field of the finder the cloud had obscured it. The nucleus set, at about 9 o'clock last night, behind the highest peak of Table Mountain (the Devil's Peak). To-night I hope we shall have better luck, but the nucleus does not look like an object permitting very accurate observation. The drift of the comet seems to be southwards; I did not, therefore, think it necessary to incur the cost of a telegram to you, but I should be glad to have your opinion for my future guidance.

I do very sincerely trust we shall get three good observations sufficient to give an orbit exact enough for the period during which I drew the tail. The boundary of the tail was exceedingly well marked. I send a rough tracing from my drawings.

*Royal Observatory, Cape of Good Hope,*
1880, *February* 10.

We have now secured observations of the comet on February 10, 11, 12, 13, 14, 15, with the following approximate results.

Comet I., 1880.

| | Cape M.T. | R.A. | N.P.D. |
|---|---|---|---|
| | h | h m | ° ′ |
| February 10 | $9\frac{1}{4}$ | 0 4 | 123 43 |
| 11 | $8\frac{3}{4}$ | 0 21 | 123 31 |
| 12 | 9 | 0 37 | 123 11 |
| 13 | $8\frac{1}{2}$ | 0 52 | 122 44 |
| 14 | $8\frac{3}{4}$ | 1 6 | 122, 10 |
| 15 | $8\frac{1}{2}$ | 1 20 | 121 34 |

On February 11, 13, and 15, we have comparison stars whose places are well determined.

*Royal Observatory, Cape of Good Hope,*
1880, *February* 17.

*On the Determination of the Personal Equation of Lunar Observers.*
By E. Neison.

In compliance with the request in Mr. Dunkin's Note in the *Monthly Notices* for January 1880 (page 166), I give a more detailed account of the method employed by me for determining the personal equations given in my Note in the previous number of the *Monthly Notices*.

Let the different observers be denoted by the Roman numerals I, II, III, . . . . and their personal equations by $e'$, $e''$, $e'''$, . . . . respectively, and as these personal equations are assumed to be liable to variations from year to year, let their values for any year $1860+k$ be denoted by $e_k'$, $e_k''$, $e_k'''$, . . . respectively.

Next let $H_k$ denote the mean error for the year $1860 + k$ of the theory embodied in Hansen's lunar tables, and let the actual error at any epoch $t$ be denoted by $H_k + h_t$ so that the quantity $h_t$ will be one which quickly changes in sign and magnitude as the time varies. Further, let $a_t$ denote the accidental error involved in the observation made at the epoch $t$. Then any observation made during the year $1860 + k$ at the epoch $t$ by the observer I will differ from the tables by the quantity

$$H_k + h_t + a_t + e_k'.$$

Consider the mean of a number $n$ of such observations

$$H_k + \frac{1}{n}\Sigma_n h_t + \frac{1}{n}\Sigma_n a_t + e_k''.$$

The quantities denoted by $a_t$ being purely accidental and as likely to be positive as negative, the mean of any number $n$ of these quantities will tend to become smaller as $n$, their number, increases. The mean may therefore be denoted by $an^{-\frac{1}{2}}$. Similarly, if the observations be impartially distributed over the period, the quantities denoted by $h_t$ will also be of opposite signs and will tend to neutralise each other, so that the mean of $n$ of these quantities will tend to become smaller as $n$, their number, increases, and may be denoted by $hn^{-\frac{1}{2}}$. If the observations be not impartially distributed, but there be a tendency for them to occur at some particular part of the lunation or year, then they will not so tend to destroy each other, and the mean will not necessarily decrease with an increase in the number of observations. In general, however, there is little tendency to such a systematic error, and if the tabular errors in the parallactic equation, apparent annual equation, and variation be corrected, there will be no such tendency.

From these considerations the mean of $n$ observations may be written in the form

$$H_k + \frac{a + \dot{h}}{\sqrt{n}} + \mathrm{E},$$

if E be put for the effect of the personal equations.

Suppose during any year $1860 + k$ there were $n$ observations made, $n'$ by observer I, $n''$ by observer II, $n'''$ by observer III, and $n^{\text{iv}}$ by observer IV. Then the mean error for the year would be

$$H_k + \frac{a+h}{\sqrt{n}} + \frac{n'}{n}e_k' + \frac{n''}{n}e_k'' + \frac{n'''}{n}e_k''' + \frac{n^{\text{iv}}}{n}e_k^{\text{iv}}. \qquad (1)$$

Similarly, the mean of the $n'$ observations made by the observer I would be

$$H + \frac{a+h}{\sqrt{n'}} + e_k'.$$

Therefore, calling the difference between these two the apparent personal error of the observer, its value is

$$\mathrm{E}_k' = e_k' - \left(\frac{n'}{n}e' + \frac{n''}{n}e_k'' + \frac{n'''}{n}e_k''' + \frac{n^{\text{iv}}}{n}e^{\text{iv}}\right) + (a+h)\sqrt{\frac{n-n'}{nn'}}. \qquad (2)$$

For each observer there will be a similar equation, so that there will be four equations to determine the four unknown quantities $e_k'$, $e_k''$, $e_k'''$, $e_k^{\text{iv}}$. If these equations were all independent it would be possible to completely determine each of these quantities in terms of the four known quantities $\mathrm{E}_k'$, $\mathrm{E}_k''$, $\mathrm{E}_k'''$, $\mathrm{E}_k^{\text{iv}}$, and the one unknown factor $(a + h)$, the sum of the mean values of the accidental errors of observation and the outstanding errors of theory. Owing to the presence of this unknown factor, it would be impossible to determine the actual personal equations with absolute accuracy.

One of these four equations, however, will be found to depend on the three others, owing to its having been used to eliminate $H_k$ from the system. To render it possible, therefore, to determine the values of the personal equations, some arbitrary condition must be assumed, so as to obtain a fourth equation. This is equivalent to assuming some standard to which to refer the various observations, as, not knowing the absolute error of the tables, it is necessary to determine this error from the observations by referring them to some standard which is supposed to give the true place of the Moon. This condition I have taken to be that

$$e_k' + e_k'' + e_k''' + e_k^{\text{iv}} = 0. \qquad (3)$$

Now, as the observations are supposed nearly equally divided amongst the four observers, it may be assumed that

$$n' = \frac{1}{4}n - m',$$

$$n'' = \frac{1}{4}n - m'',$$

$$n''' = \frac{1}{4}n - m''',$$

$$n^{\text{iv}} = \frac{1}{4}n - m^{\text{iv}},$$

when $m'$, $m''$, $n'''$, $m^{iv}$, will be four small integers, either positive or negative, and usually less than one-twentieth of the value of $n$. Substituting these values, then to determine $e_k'$, $e_k''$, $e_k'''$, $e_k{}^{iv}$, there will be the four independent equations

$$\left.\begin{aligned} E_k' &= e_k' + \frac{1}{n}\left\{ m'e_k' + m''e_k'' + m'''e_k''' + m^{iv}e_k{}^{iv} \right\} + (a+h)\sqrt{\frac{n-n'}{nn'}}, \\ &\cdot \\ &\cdot \\ E_k{}^{iv} &= e_k{}^{iv} + \frac{1}{n}\left\{ m'e_k' + m''e_k'' + m'''e_k''' + m^{iv}e_k{}^{iv} \right\} + (a+h)\sqrt{\frac{n-n^{iv}}{nn^{iv}}}. \end{aligned}\right\} \quad (4)$$

In this manner it is possible to completely determine $e_k'$ . . . $e_k{}^{iv}$ in terms of known quantities and the small unknown factor $(a+h)$, this last factor rendering their exact values slightly uncertain.

Suppose in this manner the values of $e'$ . . . . are obtained for several successive years, and that they can be represented by

$$\begin{aligned} e_k' &= e_o' + \Delta e_k', \quad . \quad . \quad . \quad . \quad . & e_k{}^{iv} &= e_o{}^{iv} + \Delta e_k{}^{iv}, \\ e_{k+1}' &= e_o' + \Delta e_k', & e_{k+1}{}^{iv} &= e_o{}^{iv} + \Delta e_{k+1}{}^{iv}. \end{aligned}$$

If the quantities denoted by $\Delta e_k'$ . . . $\Delta e_k{}^{iv}$ . . . . are small quantities, much smaller than the probable error of $e_o'$ . . . and irregular in sign and value, it is obvious that each of these may be fairly assigned to the effect of the small error introduced by the small unknown quantity depending on $(a+h)$ and arising from the outstanding errors of observation and theory. For if there were any real variation in the personal equations, then these quantities $\Delta e_k'$ . . . $\Delta e_k{}^{iv}$ . . . ought to reveal its existence by systematic variation in value, by regularly increasing and decreasing. If nothing of this kind occurs, then it may be fairly assumed that the personal equations have remained sensibly constant year after year.

Now, in the cases discussed in my paper this is actually the case. Thus, for instance, for the observers distinguished as III and IV the personal equations between 1863 and 1869 are

| | |
|---|---|
| $e'''_3 = +\cdot057 - \overset{s}{\cdot}002$, | $e^{iv}{}_3 = -\cdot082 - \overset{s}{\cdot}014$, |
| $e'''_4 = +\cdot057 + \cdot006$, | $e^{iv}{}_4 = -\cdot082 + \cdot027$, |
| $e'''_5 = +\cdot057 - \cdot004$, | $e^{iv}{}_5 = -\cdot082 - \cdot008$, |
| $e'''_6 = +\cdot057 - \cdot010$, | $e^{iv}{}_6 = -\cdot082 - \cdot009$, |
| $e'''_7 = +\cdot057 + \cdot013$, | $e^{iv}{}_7 = -\cdot082 - \cdot008$, |
| $e'''_8 = +\cdot057 + \cdot004$, | $e^{iv}{}_8 = -\cdot082 - \cdot003$, |
| $e'''_9 = +\cdot057 - \cdot008$, | $e^{iv}{}_9 = -\cdot082 + \cdot006$. |

In finding then the personal equations of these observers, it

may justly be assumed that their personal equations remained constant throughout this period, and all seven years may be grouped together to determine them. Moreover, as the personal equations have remained constant, the standard to which they have been referred must have remained constant, as it is derived from the condition

$$e_o' + e_o'' + e_o''' + e_o^{\text{iv}} = 0, \tag{5}$$

As long as the observations are made by the same four observers, the preceding method can be adopted without alteration. Now, suppose in the year $1860 + j$, one of the four observers, say I, is replaced by a new observer, say V; then throughout the quantity $e'$ will be replaced by $e^{\text{v}}$. The method of determining the personal equations will remain unaltered, but they will now be referred to the standard derived from the condition

$$e_j'' + e_j''' + e_j^{\text{iv}} + e_j^{\text{v}} = 0, \tag{6}$$

and not, as before, from the condition that

$$e_o' + e_o'' + e_o''' + e_o^{\text{iv}} = 0.$$

It will therefore be necessary to take into account the effect of this change in the standard to which the observations are referred.

Now, suppose these two standards differ by a small quantity $c$, and that $(e_j'')$ $(e_j''')$ $(e_j^{\text{iv}})$ . . . represent the personal equations as determined by the new standard (6); then each of these will be greater than its true value referred to the original standard by the quantity $c$. Therefore $c$ can be determined by the equation

$$e_o'' + e_j''' + e_o''' = (e'') + (e_j''') + (e_j^{\text{iv}}) + 3c,$$

$$c = \frac{1}{3} \frac{(e_o'') + e_o''' + e_o^{\text{iv}}}{(e_j'') + (e_j''') + (e_j^{\text{iv}})}, \tag{7}$$

provided it be assumed that no change has occurred during the year in the personal equations of these three observers. This assumption is reasonable enough. If they have been constant for several years previous to this, it is unlikely that they should change at this very time that a new observer is introduced, as the mere change of observers cannot give rise to any such variation. It is of course possible that one observer may have chanced to change his personal equation at this very epoch; but, even if this be so, only one-third of this change will be thrown on the determination of the difference between the two standards: this risk is unavoidable. Even, however, if one observer had changed his personal equation by the small increment $x$, then $c$ would undergo a fictitious increase of $\frac{1}{3}x$; and when the personal equations came to be compared with those of previous years, whilst two would seem to have decreased by $\frac{1}{3}x$, the third, which had really varied, would seem to have increased by $\frac{2}{3}x$.

If, therefore, only one had varied, this would serve to indicate which with some degree of probability. For it is much more probable that one should have varied than that two should have done so and in the same direction.

It has been already shown that it is possible to determine with some certainty whether the personal errors of the observers have changed during any group of years $k, k+1, \ldots j-1$, through which period the observers have remained the same. Suppose a break to occur at the beginning of the year $j$, through one observer being replaced by another, and that this fresh arrangement remains unaltered for the group of years $j, j+1, \ldots l-1$; then it can be ascertained in exactly the same manner whether the personal equations of the observers have remained unchanged for the period between the years $j$ and $l$. Suppose during both these groups they remain unaltered, then the difference between the two standards can be determined with considerable approximation by the comparison of the mean results for each group of years in the manner already detailed. The difference—denoted by $a$ in my paper—between these two standards having been determined, it is possible to reduce the personal equations for the entire period covered by both groups to any standard which may be assumed.

The same process may then be extended to a third group of years commencing with the year $l$ in which yet another old observer is replaced by a new observer, and in this manner a third set of personal equations determined and reduced to the same standard as before.

It is in this manner that the four quantities $a_1$, $a_2$, $a_3$, $a_4$, were determined which were given in my former paper, and by means of which I was able to determine the personal equations of the different observers, *without* being obliged to assume either that these personal equations remained constant, or even that the personal equations of any one observer must be assumed to remain constant. It so happens that I find the personal equations of the older observers to be constant, so that their effect can be accurately computed for the years between 1862 and 1873. The later observers seem to have personal equations liable to variation, or through less experience are liable to greater accidental errors of observation. In my former paper, having practically only two groups, each of two years' duration, from which to determine two new personal equations, the results were less accurate than could be desired. When, however, the observations for 1877, 1878 and 1879 are available it will be possible to arrive at more definite results on this most important matter.

I think the detailed account given above will remove any doubts which Mr. Dunkin may have entertained as to the soundness of the method by which were determined the personal equations given in my paper. It is obvious that, admitting that Mr. Thackeray's personal equation may be liable to vary or its value may be founded on too few observations, these things can have

no influence in vitiating the results arrived at by me, as the results for the other observers do not depend in the least on the value assigned by me to Mr. Thackeray's personal equation. At the same time I would remark that in my paper I point out that the results for Mr. Downing, and more especially for Mr. Thackeray, rest on so few observations that they must be considered uncertain.

Mr. Dunkin, in the concluding paragraph of his Note, gives the results of his determination of the personal equations of Messrs. Lynn, Downing and Thackeray, as compared with Mr. Criswick for the three years 1877 and 1878, derived from data inaccessible to me. Assuming Mr. Criswick's error to have remained unaltered, these results transformed into my own standard show that the personal equations of Mr. Downing and Mr. Thackeray have changed since 1876. The correction to be applied to the mean of the observations of both limbs, in order to reduce Mr. Downing's and Mr. Thackeray's observations to my standard is given in the following table together with those derived from the data supplied by Mr. Dunkin.

| | 1875 | 1876 | 1877 | 1878 |
|---|---|---|---|---|
| | s | s | s | s |
| Mr. Downing | = −0·086 | −0·097 | +0·033 | −0·017 |
| Mr. Thackeray | = −0·108 | −0·130 | +0·033 | −0·098 |
| Mean effect on year | −0·057 | −0·054 | +0·020 | −0·020 |

The mean effect on the year is derived by referring the personal equations to the standard adopted by me and taking into account the number of observations made by each observer. The above results show that the change in the personal equations will produce an apparent diminution of the tabular error of the Moon amounting to about 1″·2 in 1877 and 0″·6 in 1878. Now, this is exactly what is shown by the observations, the apparent decrease in the tabular errors for these years being 1″·3 for 1877 and 0″·9 for 1878, taking into account the effect of the apparent decrease in the tabular error due to the change in the nature of the systematic errors of long and short period. So far, therefore, from the results given by Mr. Dunkin invalidating the conclusions arrived at in my paper they tend to strongly confirm them.

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*Supplementary Note on the Changes in the Error of Hansen's Lunar Tables.* By W. T. Lynn, B.A.

This Note is simply a supplement to the paper printed at page 82 of the present volume (*Monthly Notices* for December 1879), giving the completion of the mean errors in longitude up to the end of last year, as deduced from the Altazimuth observations. And, at the close of my superintendence of that instru-

ment, I have to thank the Astronomer Royal for the permission to communicate these results.

In the paper just referred to, I gave the mean error in longitude for the first three quarters of last year, and I now add that for the last quarter, putting the four together, as follows :—

| | Mean Error in Longitude. | No. of Observations. |
|---|---|---|
| 1879, January to March | +8″·25 | 28 |
| April to June | +8·69 | 42 |
| July to September | +9·48 | 47 |
| October to December | +9·48 | 38 |

The mean result for the year, therefore, is + 9″·04 from 155 observations.

The following Table exhibits the mean error for each successive year of the last four years :—

| Year. | Mean Error in Longitude. | No. of Observations. |
|---|---|---|
| 1876 | +9″·31 | 171 |
| 1877 | +8·25 | 182 |
| 1878 | +7·48 | 161 |
| 1879 | +9·04 | 155 |

The diminution to a turning-point, and subsequent return to an increase, are sufficiently obvious. In consequence of my long illness in 1878, I did not observe after the month of April in that year, and only occasionally in 1879. A considerable number of observations was therefore made in those years by the computers, so that the observers were more numerous than usual, and the mean results are likely to be more than usually independent of personality. I may remark that had the rate of increase to which I called attention in my first paper on the subject continued, the mean error in longitude in 1879 would have been about 12″, differing from the actual mean error by a larger quantity than can well be accounted for by personality in observing.

*Blackheath,*
1880, *February* 2.

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*On the Longitude of the Observatory, Windsor, New South Wales.* By J. Tebbutt, Esq., Director of the Observatory.

The longitude $10^h\ 3^m\ 15^s{\cdot}7$ E. hitherto adopted for my Observatory depends on that derived for the Sydney Observatory by the Rev. W. Scott from corresponding moon-culminations observed at Greenwich and Sydney and the Cape of Good Hope and Sydney, the difference of longitude between Sydney and Windsor

having been approximately determined in 1865 by telegraph as $1^m\ 30^s{\cdot}0$. In the middle of 1878 I discussed twelve occultations of well-determined stars observed by me at Windsor and presented the results to the Royal Society of N. S. Wales. The Moon's tabular places were corrected by observations made at Greenwich about the dates of the occultations. It appeared from the discussion that the longitude of both the Sydney and Windsor Observatories, *east* longitude being regarded as *positive*, required a correction of $+6^s{\cdot}84$. I have since extended my investigation to eight additional occultations, and have now much pleasure in communicating the results to the Society. The geographical latitude of my Observatory as deduced from ninety-three prime vertical observations of well-determined stars culminating within 39′ of the zenith is $-33°\ 36'\ 28''{\cdot}9$. Adopting Bessel's ratio of the Earth's axis, the corresponding geocentric latitude and logarithm of the Earth's radius will be $-33°\ 25'\ 53''{\cdot}0$ and 9·9995576, and these with the assumed longitude $10^h\ 3^m\ 15^s{\cdot}7$ E. are the constants employed in the reductions.

The mean places of the occulted stars have been derived from numerous modern Catalogues. The Moon's places have been interpolated with second differences from the hourly Ephemeris of the *Nautical Almanac* and corrections based on observations at Greenwich, Radcliffe, and Washington applied. The corrections for the occultations in 1876 depend on Greenwich alone, and were kindly communicated to me in MS. by the Astronomer Royal. The accompanying table exhibits the dates and local mean times of the occultations, the adopted corrections of the Moon's tabular places and the resulting corrections to the assumed longitude. No corrections have been applied to the Moon's tabular parallax and semidiameter. I may state that the occultations of *m* Tauri, 3 Sagittarii, and both phases of the occultation of Antares on July 3, 1876, were very unfavourable for longitude determinations. The mean of the twenty corrections is, however, $+6^s{\cdot}15$, which gives for the longitude $10^h\ 3^m\ 21^s{\cdot}85$ E. Fifty-three transits of the Moon, comprising fifty-one of the first limb and two of the second, compared with the hourly Ephemeris of the *Nautical Almanac* corrected by means of the Washington transit-circle observations give $10^h\ 3^m\ 21^s{\cdot}58$ E. for the longitude. It will now be interesting to compare the occultation and moon-culmination results with the longitude of my Observatory as deduced from the longitudes of the Sydney and Melbourne Observatories combined with telegraphic differences. The difference of longitude of the Sydney and Windsor Observatories has been recently found by careful telegraphic determinations to be $1^m\ 28^s{\cdot}83$, no allowance, however, having been made for personal equation. The longitude of the Melbourne Observatory from a large number of moon-culminations is $9^h\ 39^m\ 54^s{\cdot}80$ E., and that of the Sydney Observatory as given by Mr. Russell in a recent paper read before the Royal Society of N. S. Wales, and also derived from moon-culminations, is $10^h\ 4^m\ 50^s{\cdot}81$ E. The tele-

graphic difference of longitude between the Sydney and Melbourne Observatories (see Mr. Russell's paper just cited) is $24^m\ 55^s{\cdot}77$; consequently, we have the corresponding results for my Observatory as follows:—

| | h | m | s |
|---|---|---|---|
| Longitude of the Sydney Observatory | 10 | 4 | 50·81 E. |
| Telegraphic difference of longitude between Sydney and Windsor | − | 1 | 28·83 |
| Longitude of my Observatory | 10 | 3 | 21·98 E. |
| Longitude of the Melbourne Observatory | 9 | 39 | 54·80 E. |
| Telegraphic difference between Melbourne and Sydney | + | 24 | 55·77 |
| „ „ „ Sydney and Windsor | − | 1 | 28·83 |
| Longitude of my Observatory | 10 | 3 | 21·74 E. |

We have thus four determinations, the mean of which is $10^h\ 3^m\ 21^s{\cdot}8$ E., and this value I propose to adopt for the future as the longitude of my Observatory.

| Date of Occultation. | Star occulted. | Windsor Mean Time of Observation. h m s | Phase of Occultation. | Adopted Corrections to Moon's Tabular Place. R.A. s | N.P.D. " | Resulting Correction to Longitude. s |
|---|---|---|---|---|---|---|
| April 18, 1866 | B.A.C. 1468 | 6 14 36·2 | D | +0·02 | +0·1 | + 7·55 |
| Oct. 15, 1866 | „ 6267 | 7 21 22·3 | D | −0·16 | +2·0 | +14·27 |
| Feb. 27, 1868 | $\mu$ Piscium | 7 48 46·9 | D | −0·06 | −0·7 | +10·04 |
| Mar. 2, 1868 | $m$ Tauri | 10 11 27·2 | D | +0·14 | +0·1 | +17·42 |
| Feb. 17, 1869 | $\xi'$ Ceti | 8 3 32·6 | D | +0·01 | −0·4 | +12·09 |
| Feb. 24, 1869 | $\delta$ Cancri | 8 0 49·3 | D | −0·24 | 0.0 | − 1·35 |
| Feb. 11, 1870 | $\zeta$ Tauri | 8 18 1·5 | D | −0·14 | +0·5 | + 3·51 |
| Nov. 30, 1870 | $\psi'$ Aquarii | 10 7 52·2 | D | −0·38 | −0·6 | +11·72 |
| Apr. 1, 1873 | A′ Tauri | 6 53 44·3 | D | −0·42 | +1.1 | + 5·45 |
| Sept. 2, 1873 | $\sigma$ Sagittarii | 13 27 17·2 | D | −0·49 | +0·8 | + 6·79 |
| Feb. 27, 1874 | $\psi'$ Cancri | 10 53 58·7 | D | −0·61 | −2·7 | + 9·61 |
| May 30, 1874 | $\delta$ Scorpii | 11 36 34·9 | D | −0·48 | −3·8 | + 5·64 |
| Apr. 15, 1875 | $\eta$ Leonis | 9 16 47·9 | D | −0·56 | −2·8 | + 3·06 |
| May 22, 1875 | 3 Sagittarii | 11 19 37·3 | D | −0·52 | −1·4 | + 3·10 |
| Sept. 7, 1875 | Antares | 7 11 17·7 | R | −0·29 | −0·6 | + 2·41 |
| Sept. 9, 1875 | B.A.C, 6220 | 8 24 3·7 | D | −0·29 | −0·6 | + 7·81 |
| June 2, 1876 | Spica | 12 34 7·2 | D | −0·68 | −5·8 | + 3·06 |
| June 2, 1876 | Spica | 13 39 5·2 | R | −0·68 | −5·8 | − 3·54 |
| July 3, 1876 | Antares | 12 14 41·0 | D | −0·76 | −3·2 | + 6·07 |
| July 3, 1876 | Antares | 13 7 32·9 | R | −0·76 | −3·2 | − 1·79 |
| | | | | | Mean Correction | + 6·15 |

*Windsor, N. S. Wales,*
1880, *January* 6.

*Mean Heliographic Latitude of Sun Spots for the Years 1874 to 1879, deduced from Photographs taken at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The numbers given in the accompanying table have been formed as follows:—

The Heliographic Latitude of each spot for each day of observation has been multiplied by its area for the day, and the sum of the products for Spots North of the Sun's Equator has been divided by the sum of the corresponding areas to form Mean Hel. Latitude of Spotted Area North of Equator. Similarly for Spots South of the Equator. In forming the Mean Hel. Lat. of entire Spotted Area the algebraic sum of the products for spots north and south of the equator has been divided by the sum of the areas; and for the Mean Distance from the Equator for all spots, the numerical sum of the products without regard to the sign of the latitude has been similarly divided.

The mean areas have been formed by dividing the sum of the daily areas by the number of days of observation.

All these means have been formed for each rotation from 1874, April 16, to 1879, Dec. 27, and from these quantities the annual means given in the table have been deduced. The detailed results for each rotation will be printed in the Greenwich Observations for 1879.

The table shows that the outburst of spots which commenced in the latter half of 1879 was marked by an increase in the mean latitude N. and S. of the spotted area. This is in accordance with Dr. R. Wolf's observations that the mean distance from the equator is least at the epoch of minimum and greatest at that of maximum of sun-spots.

| Year. | Spots North of the Equator. | | Spots South of the Equator. | | Mean. Hel. Lat. of entire Spotted Area. | Mean Distance from Equator of all Spots. |
|---|---|---|---|---|---|---|
| | Mean Area. | Mean Hel. Lat. | Mean Area. | Mean Hel. Lat. | | |
| | | ° ′ | | ° ′ | ° ′ | ° ′ |
| 1874 | 245 | + 9 3 | 326 | −12 9 | −2 57 | 10 48 |
| 1875 | 125 | +11 12 | 127 | − 9 50 | +1 49 | 10 35 |
| 1876 | 43 | +12 31 | 84 | −10 55 | −4 18 | 11 22 |
| 1877 | 32 | + 9 10 | 60 | − 9 41 | −4 17 | 9 32 |
| 1878 | 21 | + 7 13 | 3 | − 7 40 | +5 40 | 7 19 |
| 1879 | 11 | +23 54 | 34 | −22 39 | −9 11 | 23 1 |

The Mean Areas, &c., for the year 1874 refer to the period of nine months from 1874, April 16, to 1875, Jan. 20.

*Royal Observatory, Greenwich,*
1880, *March* 11.

## *On the Systematic Errors of the Greenwich North Polar Distances.*

By W. H. M. Christie, Esq.

(*Abstract.*)

The long series of observations of N.P.D. made at the Royal Observatory, Greenwich, under the superintendence of Sir G. B. Airy from 1836 to 1878 is discussed in this paper with reference to the systematic errors in the flexure and R−D corrections, in the refractions and in the adopted latitude. It is pointed out that the coefficient of refraction cannot be determined with sufficient accuracy from circumpolar stars, since refraction produces on stars at different distances from the pole only a differential effect which is comparatively small as far as Z.D. 75°, at which point the law of refraction begins to be somewhat uncertain; and that this effect is masked by uncertainty in the flexure and by the probable errors of the observations. On this view the determination of refraction must depend mainly on observations of southern stars made at Greenwich and at southern observatories, for which the effects of refraction are additive.

In the series of observations 1836-1878 the instrumental conditions have been twice changed, involving changes in the horizontal flexure and R−D correction, and from this it is inferred that the R−D correction is probably connected in some way with flexure, and not with any abnormal refraction.* The observations may be divided into three periods: (1) 1836–1849 made chiefly with the Troughton Mural Circle, for which there was no flexure or R−D correction; (2) 1851–1865, with the Transit-Circle before the central cube was perforated: the flexure correction was + and the R−D correction − for south stars; (3) 1866–1878, with the Transit-Circle after the central cube was perforated: the flexure correction was −, and the R−D correction + for south stars. After the piercing of the cube the observations of the collimators in the determination of flexure were made through the cube, instead of with the Transit-Circle raised. A table is given of the annual results for co-latitude and corrections to the position of the ecliptic: and in order to make these comparable they are corrected so as to reduce them to the same refractions (Bessel's *Tabulæ Regiomontanæ*) throughout; and in the case of the result for the ecliptic to the same co-latitude 38° 31′ 21″·90 and to Le Verrier's value of the obliquity.

The latitude determinations from circumpolar stars are discussed in four groups:—(1) 1840–1847, with the Troughton Circle; (2) 1851–1861, with the Transit-Circle, when the old law $a + b \sin z$ for R−D was used; (3) 1862–1865, with the Transit-Circle, when the new law $a + b \sin z \cos^2 z$ was used;

* In a paper on the "Discordance of Direct and Reflexion Observations" (*Memoirs*, vol. xxxii.) the Astronomer Royal concluded from a discussion of the latitude determinations 1836–1860 that the R−D correction probably depended on the shutter opening; but this was written before the piercing of the Transit-Circle cube.

(4) 1868–1876, with the Transit-Circle after the piercing of the cube, when the diminution in Bessel's refractions ('00531 × refraction) proposed by Mr. Stone was adopted. Tables and curves are given showing the excess of N.P.D. above the Pole, or twice the error of assumed co-latitude, from groups of stars at different distances from the Pole for these four periods, with Bessel's refractions and the old and new laws for R−D respectively, and with Mr. Stone's diminished refractions and the new law for R−D. It is pointed out that for the first three periods, 1840–1847, 1851–1861, and 1862–1865, Bessel's refractions and the old law for R−D on the whole give better results at any rate as far as N.P.D. 40°. But as Mr. Stone had based his diminution of the refractions on the observations 1857–1865, these are discussed separately, giving residuals for this particular period which, though differing somewhat from those given by Mr. Stone, would be decidedly improved by a diminution of the refractions. The amount of this diminution depends to some extent on the adoption of the new law for R−D: with the old law it would be only one-half that found by Mr. Stone. But the strength of this evidence in favour of diminished refractions is shaken somewhat by the results for the longer period 1851–1861, and still more by those with the Troughton Circle, 1840–1847, which are unaffected by flexure and R−D corrections. Further, it is pointed out that Mr. Main's exhaustive discussion of the observations 1836–1854 (*Memoirs*, vol. xxvi.) shows that as far as Z.D. 82° Bessel's refractions unaltered represent satisfactorily the observations of northern as well as of southern stars, and that down to this limit no correction of Bessel's refractions of the *Tabulæ Regiomontanæ* is admissible for these observations.

The results for the period 1868–1876 indicate such a large error at N.P.D. 20° (where the curve runs up almost to a cusp) that no certain conclusion can be drawn from them as regards the refractions. Slightly more accordant results are given with Bessel's refractions unaltered and the new law for R−D. If the observations above Pole at N.P.D. 20° (where the R−D correction is well determined) are assumed to be free from error, a correction of nearly + 1″ would be required at Z.D. 60° N. to the observed zenith distance (numerical).

A comparison is next made of the co-latitudes deduced from observations of circumpolar stars and of the Sun, the latter being inferred from the ecliptic investigation in which the correction to the mean of the observed N.P.D.'s of the Sun throughout the year is given. The following are the results corrected for error of thermometer:—

| | With Bessel's Refractions. Co-latitude from stars. | from Sun. | With Stone's Refractions. Co-latitude from stars. | from Sun. |
|---|---|---|---|---|
| 1836–1849 | 21″·85 | 21″·90 | 21″·61 | 22″·40 |
| 1851–1865 | 21·87 | 21·27 | 21·63 | 21·77 |
| 1866–1878 | 21·83 | 21·84 | 21·59 | 22·34 |

The balance of evidence is by a majority of 2 to 1 against any diminution of the refractions. But in the period 1851–1865 there is a large discordance in the results from the Sun, which seems to indicate an error at Z.D. 60° S. before the piercing of the cube, analogous to that at Z.D. 60° N. shown by circumpolar stars after the piercing.

A table is given exhibiting the comparison of the various Greenwich Catalogues with the Cape (1860) and Melbourne (1870) Catalogues in extension of that given by Mr. Downing (*Monthly Notices*, vol. xxxix. p. 133) for the first Seven Year Catalogue. It is pointed out that the use of Mr. Stone's diminished refractions in the Nine Year Catalogue has produced intolerable discordances, which are removed by the substitution of Bessel's (with Main's corrections below Z.D. 82°), and that the comparisons of the Greenwich first Seven Year Catalogue (1854–1860) with the Cape and Melbourne catalogues seem to show that a negative correction is required to the Z.D.'s south of the zenith, such as appeared to be indicated by the observations of the Sun 1851–1861.

The question of the obliquity of the ecliptic is discussed with special reference to the value of the secular variation. Bessel found the value $-0''\cdot457\ t$ from observations 1755–1815, whilst Le Verrier adopted $-0''\cdot476\ t$ based entirely on his theory of the Sun, rejecting the value $-0''\cdot4576\ t$ which he had previously found from observations 1755–1846. Under these circumstances the following comparison has been made of Le Verrier's and Bessel's formulæ respectively with the Greenwich results :—

| | With Old Law For R−D. Corrections to | | With New Law for R−D. Corrections to | |
|---|---|---|---|---|
| | Le Verrier. | Bessel. | Le Verrier. | Bessel. |
| | ″ | ″ | ″ | ″ |
| 1836–1849 | +0·21 | +0·22 | +0·21 | +0·22 |
| 1851–1865 | +0·16 | −0·11 | +0·32 | +0·05 |
| 1866–1878 | +0·20 | −0·40 | +0·04 | −0·56 |

It appears from these quantities that the Greenwich observations are well represented by Le Verrier's value of the secular variation and not by Bessel's, and that the old law for R−D, $b \sin z$ gives more accordant results than the new law, $b \sin z \cos^2 z$. It is pointed out that the constant correction $+0''\cdot19$ to Le Verrier's obliquity given by the Greenwich observations represents very nearly the effect of the difference $(+0''\cdot26)$ between the refractions of the *Fundamenta* (which Le Verrier appears to have used) and those of the *Tabulæ Regiomontanæ* now used at Greenwich.

In conclusion it is urged that every effort should be made to obtain observations of stars by reflexion as near the horizon as practicable in order that the R—D correction may be determined for large zenith distances, and that the determination of horizontal flexure should be checked by other methods, since the adop-

tion of the old law, $b \sin z$ for the R—D correction, virtually neutralising the flexure correction, would imply that the latter was erroneous. As regards the question of refraction, a comparison is given between the temperatures of the air at different altitudes as observed by Mr. Glaisher in his balloon ascents and those assumed by Laplace, Bessel and Lubbock in their theories of refraction; and the inference is drawn that Bessel's refractions may be too large for large zenith distances, through his assumption of an erroneous law for the constitution of the atmosphere, though they may be sensibly correct as far as about Z.D. 80°.

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*Description of a proposed new Uniform Pressure Clock.*
By T. Buckney, Esq.

The object of the present paper is to submit to the consideration of the Royal Astronomical Society a means of preventing the changes in the density of the atmosphere from reaching a clock and affecting its rate, by enclosing the clock in an air-tight case in which a uniform pressure may be maintained.

The late Mr. Carrington endeavoured to attain the same end by having a case of copper made for a clock of the ordinary construction; but he does not appear to have been entirely successful. The necessity of winding the clock with a winder passing through a stuffing-box seemed to be a source of frequent trouble, and the repeated breaking of the plate glass covering the dial was also an annoyance. I venture to think the method I am about to describe promises a better result.

I propose, in the first place, to mount the clock movement and hang the pendulum on a massive bracket projecting like a shelf from the wall to which it is fixed; covering both with a bell-glass like the receiver of an air-pump.

To the under side of the shelf I propose to fix a similar bell-glass, but one so elongated as to take in and enclose the pendulum. The upper and under surfaces of this shelf, which would form the seat-plate of the clock, would be worked to a true plane, and the bell-glass covering the movement would have a ground edge, so that the joint made here would be air-tight. It would probably be very difficult to grind the edge of the lower glass on account of its depth, therefore it would be better to fasten to it, by a suitable cement, a metal ring previously worked to a true surface, and thus obtain an equally good joint with the under side of the seat-plate. The space thus enclosed by these glass vessels would form the clock-case, and would be placed in connection with an air-pump by a tube screwed into the seat-plate. Now, setting aside for the moment the necessity of winding, and assuming the above provisions to be successfully carried out, we should have a clock going in an air-tight case in which the pressure may be

reduced to and maintained at any desired point. A barometer tube and a thermometer placed in the case would enable the pressure and temperature to be readily seen.

I will now show how the clock may be kept wound without interfering with the state of things thus far produced. Almost all Observatory clocks, such as this would be, are provided with some means of making galvanic contact every second, this seconds' contact being used for the purpose of pricking on a chronograph barrel, working electric dials, or controlling other clocks—in fact, for distributing the time shown by the standard clock to other parts of the building. At the Royal Observatory, Greenwich, this is done through the agency of a "relay" which, itself worked by a feeble current sent through the clock-springs, sets in motion, simultaneously, three other distinct and more powerful currents, which convey the time to the distant instruments. I propose, therefore, that the clock we are considering shall, in like manner, make a seconds' contact, and work a similar relay: the wires connecting the relay with the clock springs passing through the seat-plate. Now, one of the currents emanating from the relay I propose to take back into the clock-case, and allot to it the task of winding the clock. This it would do by means of a small electro-magnet attracting, every time the current passed—that is, every second—an armature provided with a click which would act upon a ratchet wheel connected with the winding work. Thus the clock would not be an electric clock which any failure of the current would stop and render useless: it would be a clock actuated by a weight as usual; but this weight would be wound up every second by the current set in motion by the clock itself. It would be automatic, assuming the current to pass with regularity: but in the event of the current ceasing, the clock would continue to go until the weight had run down; and even then it might be wound from the outside by means of a hand "contact-breaker" introduced into the circuit which does the winding. But let us suppose that a failure has taken place in the current of the primary circuit, as we may call the one by which the relay is worked, that being the only one with which we need deal, seeing that any defect in the subsidiary ones could be remedied outside and independently of the clock; and let us consider what amount of inconvenience would be caused by any such failure. Now, setting aside as improbable such an accident as a severance or disconnection of the wires, a failure must, I conceive, arise from one of two causes: either the battery must have ceased to generate a current, in which case a new one would have to be substituted for it at no greater inconvenience than is experienced under existing circumstances; or there must be a stoppage of the current by the oxidation and fouling of the contact-springs, which would then require cleaning, for which purpose access to the clock must be had, and the uniformity of the pressure disturbed. But even from this no great harm would, I think, arise. It is assumed that the normal pressure in the clock-case would be but a little less than the lowest

atmospheric pressure of the locality, so that the danger of leakage might be reduced to a minimum. To remove the contact-springs from the clock all that would be necessary would be to let in the air; lift off the upper bell-glass; pull out the slide on which the springs are mounted; do what might be found necessary in the way of cleaning; replace them in their position; put on the bell-glass cover again, and reduce the pressure to its normal point. As I have said, I cannot conceive that such an operation as this should necessarily cause any great or permanent disturbance in the clock's rate. But in any case the contact-springs would require cleaning only at long intervals. The great trouble with galvanic contacts has arisen from the fusing and oxidation of the platinum points by the spark of the induced current produced at the breaking of the circuit. A simple means of entirely preventing this spark is now known, and there is no longer this difficulty. I believe the galvanic contact-springs of the Greenwich Standard Sidereal Clock have been working with the greatest regularity for five months, and it has not yet been found necessary to clean them: what is done at Greenwich in this way may, it is obvious, be done elsewhere with equal success.

We will now return to the winding, which, it will be remembered, is to be done every second. Now, in order that this constant winding may in no way interfere with the good going of the clock, but that the force acting on the clock train may be constantly uniform, I propose to use the arrangement known as the "endless chain," although in this instance it would probably be an endless line of silk of uniform thickness, which I think would answer perfectly. Sufficient fall for the weight should be allowed for the clock to go for two days after a cessation of the winding.

In such a clock any desired pendulum or escapement may be used. The clock should be fixed in such a position that a uniform temperature should be maintained, if possible; and, in order to facilitate this, I would envelop the glass vessels in an outer casing of thick wood lined with felt or other non-conducting material. Unless the air were entirely removed from the clock case—a state of things very difficult to maintain for any length of time—it is evident that changes of temperature would produce corresponding changes of pressure: and although these could, no doubt, be easily corrected by a proper allowance for them in the thermal compensation of the pendulum, as are the other more important temperature effects, still it would, I imagine, be better to avoid these errors. In any case, sudden changes of temperature ought to be carefully guarded against: they do a certain amount of mischief before the compensation is brought into operation.

I may state in conclusion, in order to avoid the charge of appropriating to myself and taking credit for the inventions of others, that the "endless chain"—the earliest form of maintaining power—was invented by the Dutch philosopher Huyghens in

the 17th century: and that the general plan of the air-tight case is almost identical with that used by Mr. Baily in his pendulum experiments; although I was not aware of this until quite recently. The method of winding is, I think, the only thing which is new: if, therefore, any merit attaches to the arrangement herein described, it is due rather to the aggregation of means conducing to the general result than to any special point of detail.

1880, *January*.

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*Notes on "A Catalogue of* 10,300 *Multiple and Double Stars &c.," forming Vol. XL of the Memoirs of the Royal Astronomical Society.* By H. Sadler, Esq.

(Hours 0–VI.)

No. for Reference.

2. This is 316. Cephei. (Bode.)

3. Σ²(2) c.g. This star is not entered in the *Catalogus Generalis* as a double star, for Struve expressly states on the *first* page of that Catalogue: "Stellæ, quibus nullum adjectum est epitheton, ut 6. γ Pegasi, sunt stellæ vere simplices, quantum hucusque compertum est." It is one of the "Stellæ Primariæ," of which a catalogue is given on pp. xxxviii to l of the Introduction (cf. also the *Summa Catalogi Generalis*, on p. lxxx). The only claim that it has to be considered in any way a *double* star is that it is S.C.C. 2, but no reference is made to this.

8. The N.P.D. should be 79° 31′, not 79° 36′.

14. Σ.6. Add *Rej.* Rejected from the great catalogue because, being of Class iv. in distance, the companion is under the ninth magnitude.

30. Cf. No. 3. The only reason for including it in a Catalogue of Double Stars is that it is S.C.C. 6.

42. OΣ 3. This is identical with No. 62, Σ 19, there being an error of $2^m$ in the A.R. of the Catalogue of 1843. Cf. page 297 of *Catalogue revu et corrigé*, in the *Recueil de Mémoires*, &c.

57. Σ 18. In a note on No. 90. *h* 1018, Sir J. Herschel observes: "The N.P.D., description, and measures agree well with those of Σ 18, but the R.A. differs by $4^m$. Original observation examined. All perfectly regular and clearly entered. It can hardly be the same double star" (p. 132). Σ 18 *is h* 1018, there being an error of $4^m$ of R.A. and 2′ of Decl. in the Dorpat Catalogue. Cf. Σ. *P.M.*, pp. xc and cxv.

67. Σ 21. Add *Rej.* Rejected in the *Mensuræ Micrometricæ* because the primary is less than the ninth magnitude.

87. This is also σ 6.

93. Σ 26. Add *Rej.* Cf. No. 14.

120. Single, and therefore rejected in the Catalogue of 1850.

128. Single, and therefore rejected in the Catalogue of 1850.

130. The R.A. is $1^m$ in error; it should be $0^h\ 19^m\ 51^s$. Cf. *P.M.* "Notæ Criticæ," pp. xciii, cxii; also pp. 173 and 295 (reference on).

No. for Reference.

150. This is also S.C.C. 12.

151. Rejected in the Catalogue of 1850, because the distance is 4′ instead of 4″, as printed in the Catalogue of 1843.

160. This is No. 163. OΣ's place is correct.

165. "There is no double star in this place; undoubtedly an erroneous observation of No. 211.' Burnham. A reference to its observation in Catalogue IV (1030) shows this at once.

172. Single. It has no claim whatever to be considered a double star. Cf. No. 3.

181*. *h* 3042. (Read *h* 3442.) "The R.A. has been corrected for an error in R.A. of 1ʰ. J. H." (Notes, p. 132). Notwithstanding this note of Sir J. Herschel's, this star appears again as No. 545.

195. This is also De. 2. It is also a double of Secchi's, who independently discovered the duplicity of A.

210. Single. Cf. No. 3.

211. This is also σ 10.

218. For OΣ 61, read OΣ 16.

225. Cf. No. 3. It is σ 11, but as Σ¹(45) c. g. it is one of the "Stellæ Primariæ."

240. Σ 50. Add *Rej.* Cf. No. 14.

248. Cf. No. 3.

252. Σ 53. Add *Rej.* Cf. No. 14.

261. Cf. *P.M.* pp. 212, 295.

262. Σ 56. Add *Rej.* Cf. No. 14.

268. Σ 57. Add *Rej.* Cf. No. 67.

271. Σ 58. Add *Rej.* Rejected because the distance is greater than 32″.

276. Cf. No. 3. It is σ 12. P.O. 175.

278. Cf. No. 3. It is σ 13.

299. Σ 66. Add *Rej.* Cf. No. 14.

315. The R.A is 0ʰ 45ᵐ 16ˢ. Cf. *P.M.* p. cxii.

318. It is Lacaille 240, B.A.C. 246.

348. Σ 81. Add *Rej.* Cf. No. 14.

355. This is 39 Andromedæ.

356. Σ 83. Add *Rej.* Cf. No 14.

364. Σ 85. Add *Rej.* Cf. No. 14.

379. Σ 89. Add *Rej.* Cf. No. 14.

398. Σ 92. Add *Rej.* Cf. No. 14.

405. Cf. No. 3. It is S.C.C. 43.

410. "Simplex." Σ. Cf. *P.M.* pp. 213, 295.

412. Cf. No. 819.

417. Single, and therefore rejected in the Catalogue of 1850.

418. This is identical with No. 420. It is 4 P. I Piscium.

No. for Reference.

419*. Σ 95 = *h* 324. "The R.A. and the position and distance of *h* 324 agree with those of Σ 95; and an error of 10′ in N.P.D. has been assumed in *h* 324, which gives 95° 52′ instead of 95° 42′. J.H." (Note, p. 132). The Decl. given in the *Catalogus Generalis* is really 10′ in error; and the N.P.D. should be 95° 52′, not 95° 42′ as printed on page 8 of vol. xl. Cf. *P.M.* p. xciii.

429*. "Notre étoile double n'est pas 34 Ceti, comme il avait été indiqué dans le Catalogue de 1843". OΣ. *Catalogue revu et corrigé*, p. 301.

431. Single, and therefore rejected in the Catalogue of 1850.

442. This is σ34.

450. Σ 105. 1st. } Cf. *P.M.* p. xciii. The places should therefore be
455. Σ 105. 2nd. } reversed.

453. Σ 102 = No. 452, *h* 2033. Cf. Σ *P.M.* p. xc. The place of Σ 102 in the *Catalogus Novus* should be increased by $2^{m}\cdot1$, and the Decl. diminished by 2′. Cf. *P.M.* p. cxv. *M.M.* p. xxxvii.

461. Rejected in the Catalogue of 1850.

466. This is σ 36.

476. The N.P.D. should be 114° 29′.

481. Σ 116. Add *Rej.* Cf. No. 14.

494. The R.A. is $1^{h}$ too large.

495. Single. Cf. No. 3.

498. *h* 2044 = Σ 119 (No. 505) "indubié" Σ. Cf. also *P.M.* p. xciii.

511. Σ 123. Add *Rej.* Cf. No. 14.

523. Σ 126. Add *Rej.* Cf. No. 14.

527. Σ 128. Add *Rej.* Cf. No. 14.

539. The correct R.A. is $1^{h}\ 23^{m}\ 22^{s}$.

545. Cf. Note on No. 181.

594. For S.C.C. 72, read S.C.C. 62.

619. Σ 152. Rejected in the *Mensuræ Micrometricæ*.

620. Σ 151. Rejected in the *Mensuræ Micrometricæ*.

632. The R.A. should be $1^{h}\ 35^{m}\ 30^{s}$. Cf. *P.M.* p. xciii.

635. *h* 643 = No. 643, Σ 160. Cf. *P.M.* p. xc.

644. Σ 159. Add *Rej.* Cf. No. 14.

645. Σ 161. Add *Rej.* Cf. No. 14.

656. Σ 165. Add *Rej.* Cf. No. 14.

663. Σ 167. Add *Rej.* Cf. No. 14.

671. Σ 173. Add *Rej.* Cf. No. 14.

677. This is also R(2).

678. Σ 176. Add *Rej.* Cf. No. 14.

679. Single. Cf. No. 3.

681. Σ 177. Σ[1] (162) e.g. is not Σ 177. Cf. *P.M.* p. xciii. The R.A. of Σ 177 is $0^{m}\cdot6$ less than the one given.

No. for Reference.

692. "Étoile que je n'ai pas retrouvée." OΣ. It may be that some confusion has arisen between this star and the preceding one.

697. Σ 181. Add *Rej.* Cf. No. 14.

708. Σ 184. Add *Rej.* Cf. No. 14.

716. Σ 188. Add *Rej.* Cf. No. 14.

720. Σ 187. Add *Rej.* Cf. No. 14.

721. Σ 190. Add *Rej.* Cf. No. 14.

728. This is $\sigma$ 50.

743. Σ 198. Add *Rej.* Cf. No. 271.

752. Σ 203. Add *Rej.* Cf. No. 67.

760. This is $\sigma$ 52.

764. Σ 209. Add *Rej.* Cf. No. 271.

771. Σ 210. Add *Rej.* Cf. No. 14. (The R.A. given in the *Catalogus Generalis* is correct.)

772*. Σ 211. Add *Rej.* Cf. No. 14.

783. Cf. No. 3. It is S.C.C. 85.

792. This is S.C.C. 86.

798. Σ 220 is identical with No. 794, Σ 218. The place of the latter is the correct one. Cf. *P.M.* p. cxv, *M.M.* xxxiv.

809. This is Hh 55.

819. "$\eta$ Arietis. Not double." Burnham. It is R(2), not R2.

822*. Single, and therefore rejected in the Catalogue of 1850.

833. This is identical with No. 860, Σ 246.

843. Single. Σ observes in the preface to his "Catalogus 795 Stellarum Duplicium ex diversorum astronomorum observationibus congestus, &c." (*Dorpat Obs.* III. p. xiv.):—"[Stellæ] quibus autem asteriscus appositus est, eæ seu in cœlo non ita inventæ sunt, seu duplices non apparuere, ulterioremque partim poscunt disquisitionem." The following stars contained in the first six hours of Vol. XL. fall under this category, not being included in other double star catalogues:—Nos. 520, 813, 1238, 1914, 2125 (Mayer), 2256, and 2325.

850. This star is single, and is the star 65 Arietis (Bode).

852. This star is not 65 Arietis (Bode), which is $4^s$ p and 6′ to the south. Cf. Σ. *P.M.* pp. 156, 174. It is incorrectly called 65 Arietis (Bode) in the *M.M.* p. 89.

855. Σ 242. Add *Rej.* Cf. No. 271.

856. Σ 243. This is identical with No. 889, Σ 256; there being an error of $5^m$ in the R.A. of Σ 243 in the *Catalogus Novus.* Cf. *M.M.* p. xxxiv, *P.M.* p. cxv.

864. Σ 247. Add *Rej.* Cf. No. 67.

882. Σ 253. This is identical with No. 897, Σ 258; there being an error of $2^m$ in the R.A. of Σ 253 in the *Catalogus Novus.* Cf. *M.M.* p. xxxiv, *P.M.* p. cxv.

886. Σ 255. Add *Rej.* Cf. No. 67.

No. for Reference.

887. This is Piazzi ii. 61, S.C.C. 96.

893. This star is single. It is entered in the *C.G.* as "vicina ad [Σ] 255." Cf. *P.M.* pp. xciv, 174.

896. This is identical with No. 917.

900. Σ 259. Rejected in the *M.M.*

912. Σ 267. Add *Rej.* Cf. No. 67. This star is, however, not Σ 267 *rej.* Cf. Note on p. 295 of the *P.M.* and p. 174. On examining its place in the heavens Σ found that Σ 267 was about 3′n and 8ˢf this star No. 912, which is C.G. 235. See *P.M.* p. xciv.

914. The A.R. may be $1^m$ in error. Cf. *P.M.* xciv., and p. 195. Σ remarks (p. 295) that the note on p. 195 makes it pretty certain that the star observed (*C.G.* 236) can hardly be Σ 264.

925. For Σ 239 read Σ 269, and also in the note on p. 133.

926. "13 Trianguli." This star is not double, and it is not 13 Trianguli. Cf. *P.M.* pp. 156, 215.

947. This is one of Struve's "Stellæ excludendæ, quia in cœlo non repertæ sunt."

964. This is also σ 75.

1007. The N.P.D. is 1° in error, it should be 47° 53′. Burnham states that this is identical with No. 1002; OΣ thinks it is not the same. Nos. 1000, 1002, and 1007 are in the cluster 34 Messier.

1016. Σ 298. Add *Rej.* Cf. No. 14.

1032. Σ 304. Add *Rej.* Cf. No. 14.

1036. Σ 305. The N.P.D. should be 71° 20′. Cf. *P.M.* p. xciv.

1042. Σ 339. Add *Rej.* Cf. No. 14.

1049. This is σ 83, = S.C.C. 117, = OΣ 47 *Rej.*

1057. The seconds of R.A. should be 33ˢ. Cf. *P.M.* p. xciv.

1075. Σ 319. Add *Rej.* Cf. No. 14.

1077. This is one of Struve's "Stellæ excludendæ, quia examine instituto non duplices sed simplices apparuere."

1082. Σ 327. Add *Rej.* Cf. No. 14.

1120. Cf. No. 3. It is S.C.C. 125, which is the only reason for including it in a Reference Catalogue of *Double* Stars.

1125*. Σ 340. "Not found by *h* in the place indicated. J. H." (Note, p. 133.) Σ 340 is identical with Σ 345, there being an error of 1° in the Decl. of Σ 340 as given in the *Catalogus Novus.* Cf. *M.M.* p. xxxiv, *P.M.* p. cxv.

1129. This is S.C.C. 126.

1130. This is identical with No. 1132, *h* 2172 being A of OΣ 50 and a third companion.

1135. Σ 348. Add *Rej.* Cf. No. 14.

1136. Σ 347. This is identical with No. 1128, Σ 343, the place of the latter being the correct one. Cf. *P.M.* p. xcv, &c.

1141. Cf. No. 3. It is S.C.C. 125 (the companion was discovered by Schröter), and up to a year or two ago this was the only ground on

No. for Reference.

which it could have been included in a *Double* Star Catalogue. It is one of Σ's "Stellæ Primariæ," and it is on this account that it appears in the *Catalogus Generalis* as No. 309.

1143. Σ 353. Add *Rej.* Cf. No. 67.

1145. Σ 354. Add *Rej.* Cf. No. 67.

1175. Σ 365. Add *Rej.* Cf. No. 67.

1180. Σ 366. Add *Rej.* Cf. No. 67. "Sine dubio in hujus stellæ singulâ R.A., a Preussio observata, error 1′ est commissus, et α C.G. minuto temporis est minuenda." *P.M.* xciv. The R.A. should be therefore $3^h\ 4^m\ 24^s$.

1197. Σ 373. Add *Rej.* Cf. No. 14.

1212. *h* 3565. The N.P.D. should be 109° 11′.

1217. No mention of Jacob's companion.

1218. Cf. No. 3. As a "double" star it is S.C.C. 131.

1237. Σ 387. Add *Rej.* Cf. No. 14.

1246. OΣ 55. Rejected in the Catalogue of 1850.

1256. OΣ 56. Rejected in the Catalogue of 1850.

1259*. Mäd. Dorp. xi. (1). This is identical with No. 1266.

1263. Σ 399. Add *Rej.* Cf. No. 14.

1272. Σ 402. Add *Rej.* Cf. No. 14.

1273. Σ 404. Add *Rej.* Cf. No. 14.

1274. Σ 405. Add *Rej.* Cf. No. 14.

1278. The R.A. should be $3^h\ 21^m\ 52^s$, the N.P.D. 101° 44′.

1283. Σ 409. Add *Rej.* Cf. No. 14.

1284. This is *h* 334, not Σ 410; which is $1^m\ 5^s$ f and 3′ 51″ s. Cf. Burnham in *English Mechanic*, August 20th, 1875, and Σ, *P.M.* p. xciv.

1286. Σ 411. Add *Rej.* Cf. No. 14.

1289. OΣ 58. Rejected in the Catalogue of 1850, because it cannot be found. It may be identical with Σ 414, with an error of 35′ in the Decl. Cf. *Catalogue revu et corrigé.*

1293. Σ 417. Add *Rej.* Cf. No. 14.

1294. Σ, from a comparison with the observation of this star in *h* V, finds an error of $1^m$ in the R.A. of the *Catalogus Generalis*. It should be $3^h\ 26^m\ 7^s$. Cf. *P.M.* p. xciv.

1295. Σ 416. Add *Rej.* Cf. No. 14.

1324. Σ 428. Add *Rej.* Cf. No. 14.

1325. Cf. No. 3. It is S.C.C. 135.

1326. Σ 429. Add *Rej.* Cf. No. 14.

1332. Σ 432. Add *Rej.* Cf. No. 14.

1333. Σ 433. Add *Rej.* Cf. No. 271.

1343. OΣ 61. Rejected in the Catalogue of 1850. It is, however, double.

1349. Σ 441. Add *Rej.* Cf. No. 14.

No. for Reference.

1354. Single.

1362*. 1363*. "σ105=S 437 and S.C.C. 140. A doubt exists of these being separate double stars." (Note, p. 133). S.C.C. 140 is a single star, 23 Pleiadum, and is used by Smyth as a pointer to σ 105 =S 437.

1376. Σ 451. Add *Rej.* Cf. No. 14.

1385. Σ 454 is identical with No. 1369, Σ 446; the place of the latter being the correct one. Cf. Σ, *M.M.* p. xxxiv., *P.M.* p. cxv.

1386. Σ 456. Add *Rej.* Cf. No. 14.

1411*. Σ 462=*h* 2206. "The R.A. by *h* is $5^m$ less than that by Σ (in the *Catalogus Generalis*), and a note to the observation by *h* in the fourth series of observations of double stars states that it is possibly not the same double star." (Note, p. 133.) For "fourth series" read "fifth series." *h* 2206 *is*, however, Σ 462, and the R.A. of Σ in the *C.G.* is correct. Cf. *P.M.* p. xcv. *h* identifies his 2210⅓ with Σ 462, but the two stars cannot be the same, as is evident from the description of 2210⅓ in H's Fifth Catalogue.

1418. This is σ 109.

1431. Σ 467. "Non est." Σ, *P.M.* p. cxv., *M.M.* p. xxxv.

1434. Σ 468. This is identical with No. 1428, *h* 5458, there being an error of $0^m{\cdot}9$ in the *Catalogus Novus*. Σ, *P.M.* cxv.

1475. Σ 488. Add *Rej.* Cf. No. 14.

1491. Σ 492. Add *Rej.* Cf. No. 67.

1503. Σ 496. Add *Rej.* Cf. No. 67.

1508. Σ 497=*h* 672. In note, p. 133, for Σ 672 read *h* 672.

1509. Σ 499. This is *not* Σ 499. Σ's double is $20^s$ f and 4′ north of this star. Cf. *P.M.* p. ccliv.

1514. Σ 502. Add *Rej.* Cf. No. 67.

1524. Σ 507. This is identical with No. 1531, Σ 513, there being an error in the R.A. of the former in the *Catalogus Novus*.

1552. The R.A. is $4^h\ 8^m\ 12^s$. Cf. *P.M.* p. 153, and the final correction on p. xcv.

1553. This is the double companion of No. 1551.

1557. Σ 519. Add *Rej.* Cf. No. 14.

1567. This is σ 119.

1574. Σ 525. Add *Rej.* Cf. No. 67.

1596. Σ 532 is rejected in the *M.M.* Cf. No. 67. No. 1596 is *not* Σ 532, which is not in the *C.G.* but is "*vicina sequens ad* 532." Cf. *P.M.* p. xcv.

1606. Σ 539. Add *Rej.* Cf. No. 14.

1609*. Σ 541. Add *Rej.* Cf. No. 67. "This appears likely to be the same double star as No. 1608, H*h* 116, 65 Tauri; the place was incorrectly set down in the MS.; and so, perhaps, the probable identity escaped the author's notice." (Note, p. 133). Σ 541 cannot be identical with No. 1608; it is possibly identical with a little pair of De's between $\kappa^1$ and $\kappa^2$ Tauri. (*A.N.* No. 1979.)

No. for Reference.

1637. "Σ¹ (445) c.g." This is entered in the *C.G.* as "*vicina ad* 549," and is a single star.

1639. This is also Σ 10; App. I.

1641*. S 450. "This was entered in MS. in pencil, as if a doubt existed of its being really a double star." (Editors' note, p. 133.) The star is of course identical with No. 1645.

1650*. The correct R.A. is $4^h\ 19^m\ 18^s$.

1652. Σ 556 is rejected in the *M.M.* Cf. No. 14. No. 1652 is *not* Σ 556, which is not in the *C.G.*, being overlooked on account of its faintness. The star given is single. Cf. *P.M.* p. xcv.; see also note on p. 295.

1655. Σ 555. Add *Rej.* Cf. No. 14.

1660. Σ 557. The A.R. of the *Catalogus Generalis*, which is based on a single observation, is $1^m$ in error. The AR should be $4^h\ 22^m\ 3^s$. Cf. *P.M.* p. xcv.

1667. Mäd. Dorp. xi. 2. This is identical with No. 1656. *h* 679 = OΣ 84; Mädler's R.A. being $1^m\ 45^s$ in error.

1673. Σ 560. Add *Rej.* Cf. No. 14.

1679. Σ 561. Add *Rej.* Cf. No. 14.

1691. Σ 568. Add *Rej.* Cf. No. 14.

1706. Σ 573. Add *Rej.* Cf. No. 14.

1722. Σ 580. Add *Rej.* Cf. No. 14.

1724. Σ 581. Add *Rej.* Cf. No. 67.

1737*. Σ 586. "There is a doubt of the existence of this star: it may be really identical with Σ 587. J.H." (Note p. 134.) In *Memoirs*, vol. xxxv. p. 69, Sir J. Herschel remarks: "There are *not* two distinct double stars, Σ 586 and 587." Σ himself shows the identity of his 586 with 587 on p. xxxiv of the *M.M.*, there being an error of $1^m$ in the *R.A.* of 586 in the *Catalogus Novus*. Cf. also *P.M.* p. cxv.

1739. This is not Σ 585, the place in the *C.G.* being that of another star. Σ 585 is 14ˢ preceding and 5′ north of No. 1739, which is a single star. Cf. *P.M.* pp. xcv. celiv. 144, and the reference on page 295.

1745. This is identical with Σ 587, No. 1742.

1750. Σ 591. Add *Rej.* Cf. No. 67.

1751. Σ 592. Add *Rej.* Cf. No. 14.

1755. This is identical with No. 1742, which is Σ 587. HJ's place is in error. See No. 1737* *supra*.

1758. Σ 593. Add *Rej.* Cf. No. 67.

1760. The star is single. Cf. No. 3.

1763. Σ 594. Add *Rej.* Cf. No. 67.

1766. Σ 597. Add *Rej.* Cf. No. 14.

1771*. *h* 684. } "Suspected to be the same double star." (Note p. 134.) There is no doubt of this being the case.

1772*. Σ 601. } Add *Rej.* One of Struve's "in acervis."

1773. Σ 600. Add *Rej.* Cf. No. 14.

No. for Reference.

1786. 1793. 1800*. Note to No. 1800. *h* 2240⅝. "The measures agree with those of Σ 609, but *h* has two observations closely agreeing in place." (p. 134). Σ 609 is *h* 2240⅝, which is also *h* 686 (No. 1786). No. 1793 is not Σ 609, which is not in the *Catalogus Generalis*, as the star given in the *C.G.* as Σ 609 is a single star of the 7th magnitude $1^m\ 3^s$ p and 6′ 29″ south of the true Σ 609. Cf. *P.M.* p. xcvi.

1789. Σ 605. Add. *Rej.* Cf. No. 67.

1802. This is entered in the *C.G.* as "vicina ad 607," and is a single star. Σ 607 itself does not occur in the *C.G.*

1804. Σ 610. No mention of the duplicity of A discovered by De.

1807. Σ 611. Add *Rej.* Cf. No. 14.

1822. Σ¹ (487) c.g. Single. Cf. No. 3.

1830*. The R.A. is $4^h\ 46^m\ 29^s$. The N.P.D. is 115°. (Cf. Note, p. 134.)

1841. Σ¹ (498) c.g. This is entered in the *C.G.* as "vicina ad 620." It is a single star.

1852. Σ 624 = *h* 30. "The minute of R.A. of *h* is 48. The observation is correctly reduced. J. H." (Note p. 134.) Σ's place is correct.

1859. Single. Cf. No. 3.

1860. Σ 626. Add *Rej.* Cf. No. 14.

1865. Σ 628. Add *Rej.* Cf. No. 14.

1877. H*h* 145 is entered twice over, as No. 1873 and here. The correct place is that of No. 1873. It is undoubtedly Σ 630.

1886. OΣ 94. Rejected in the Catalogue of 1850.

1890. Σ¹ (512) c.g. Single. Cf. No. 3.

1902. OΣ 96. Rejected in the Catalogue of 1850.

1913. Σ 641. Add *Rej.* Cf. No. 14.

1921. Σ 642. Add *Rej.* Cf. No. 14.

1932. Σ 647. 'Simplex.' Σ, *M.M.* p. xxxv. &c. It is also S. C. C. 183.

1934. Single.

1938. Σ 650. Add *Rej.* Cf. No. 14.

1958. This is probably identical with No. 1966, there being a mistake of $1^m$ in the R.A. of one of the two pairs.

1974. Σ 660. This is identical with No. 1972, Σ 657; the place of Σ 660 being in error. Cf. *M.M.* p. xxxiv. *P.M.* p. cxv.

1976. Σ 662. "The P.D. by the Dorpat Catalogue is 64° 15′, and the same N.P.D. is to be found for this star in the *Mensuræ Micrometricæ*, p. 110; but in the *Catalogus Generalis* it is given as 94° 15′ 54″! J. H. It is entered in the present Catalogue with N.P.D. 64° 16′." (Note p. 134.) The mistake in the *C.G.* is corrected on p. xcvi. of the *P.M.* The place of Σ 662 is $5^h\ 5^m\ 30^s$ N.P.D. 64° 15′.

1994. Σ 672. Add *Rej.* Cf. No. 14.

2018. Σ 682. Add *Rej.* Cf. No. 14.

2043. This is 22 Orionis.

No. for Reference.

2057*. Σ 699. "The N.P.D. of this star was set down in the MS. from the *Catalogus Generalis* as 52° 17′ 11″, but it is printed 52° 7′ on the authority of three observations of this star made at the Radcliffe Observatory." (Note p. 134.) The faulty decl. of the *C.G.* is corrected on p. xcvi. of the *P.M.*

2074. Σ 707. The R.A. should be increased by $1^m$, and the N.P.D. diminished by 2′.

2080. Σ 703. Add *Rej.* Cf. No. 14.

2107. Σ 717. Add *Rej.* Cf. No. 14.

2119. Σ 722. Add *Rej.* Cf. No. 14.

2143. This is not Σ 733. Σ 733 precedes this star by $35^s$. Cf. *P.M.* p. ccliv. No. 2143 is a single star.

2153. Single.

2162. This star is single. It is entered in the *C.G.* as "vicina ad 743."

2165. This is also R (3).

2167. This is identical with No. 2177.

2173. This is not 133 Orionis (Bode).

2176. Σ 746. Add *Rej.* Cf. No. 67.

2177. This is 133 Orionis (Bode).

2179. This is entered in the *C.G.* as "altera ad 743," and is a single star Cf. No. 2162.

2181. This is Σ 16; App. I.

2193. Σ 756. Add *Rej.* Cf. No. 14.

2194. "Σ 757." Σ 757 is $35^s$ f. and 2.5′ n. of No. 2194, which is *C.G.* 612, and is a single star. Σ 757 is $3^s$ p. Σ 758, on the same parallel of Decl. Cf. *P.M.* p. ccliv., the note on p. xcvi. being incorrect.

2206. Σ 760. Add *Rej.* Cf. No. 14.

2220. This is identical with No. 2201, Σ 758, there being an error of $2^m$ in the R.A. of Σ 765 in the *Catalogus Novus.* Cf. *P.M.* p. cxv. *M.M.* p. xxxiv. &c., and see No. 2194.

2224. Σ 767. This is identical with No. 2315, Σ 806, there being an error of $10^m$ in the R.A. of Σ 767 in the *Catalogus Novus.* Cf. *P.M.* p. cxv, *M.M.* p. xxxiv. &c.

2231. "Σ 773." This is not Σ 773, which is $25^s$ f. and 3′ south of this star, which is *C.G.* 621, and is a single star. Cf. *P.M.* p. ccliv., and note on page xcvi.

2233. This is Leporis 45 (Bode); the place being $5^h\ 31^m\ 47^s$. 107° 59′.

2247. This is not Σ 781, which is $24^s$ f. on the same parallel. No. 2247 is *C.G.* 628, and is a single star. Cf. *P.M.* p. ccliv.

2263. Σ 789. Add *Rej.* Cf. No. 14.

2274. Single. Cf. *P.M.* p. 198. It is entered as "Anonyma 7ʰ7, præcedens 791."

2283. This is 133 Tauri.

2287. Σ 793. Add *Rej.* Cf. No. 14.

No. for Reference.

2294. Single.

2301. For N.P.D. 144° 34′ read 114° 34′.

2305. Does not exist. Cf. *M.M.* p. xxxv. *P.M.* p. cxv.

2307. Σ 801. Add *Rej.* Cf. No. 14.

2309. Σ 803. Add *Rej.* Cf. No. 67.

2310. Σ 804. Add *Rej.* Cf. No. 67.

2321. This is also De. 10.

2327. "Σ 813." This is not Σ 813, which is $0^m{\cdot}9$ *f.* No. 2327 is single. Cf. *P.M.* pp. xcvi. 158.

2330. N.P.D. 16° 1′.

2332. Σ 812. Add *Rej.* Cf. No. 14.

2336. Does not exist. Cf. *M.M.* p. xxxv. *P.M.* p. cxv.

2344. This is identical with No. 2346; the place of the latter star being the correct one.

2348. Σ[1] (656) c.g. One of the "Stellæ Primariæ." Cf. No. 3.

2360. Σ[1] (657) c.g. Cf. No. 2348.

2366. Σ 822. Add *Rej.* Cf. No. 14.

2389. Rejected in the Catalogue of 1850.

2393. Σ 827. Add *Rej.* Cf. No. 14.

2396. Σ 828. "Fortasse locus ita erroneus, ut sit eadem ac 849, errore 7′ in A.R. assumpto." Σ. *M.M.* p. xxxv. Cf. *P.M.* p. cxv. also p. 176. No. 2396 is a single star, and there is no doubt of the identity of Σ 828 with Σ 849.

2398. "Loco" Σ 828. Cf. *P.M.* p. 176. The star is single.

2405. Σ 832. Add *Rej.* Cf. No. 14.

2410*. Σ 833. Add *Rej.* Cf. No. 14. "This star was not seen by *h*, after repeated search for it. J. H." (Note p. 134.) No doubt some confusion has arisen between this object and No. 2415, and 2410 probably does not exist. R.A. of Σ 833 in *Catalogus Novus* is $5^h\ 53^m\ 5^s$. "Nisi A.R. = 54·5"?

2413. "Σ 835." This is not Σ 835, the R.A. of which is $5^h\ 53^m\ 40^s$. Cf. Σ *Cat. Novus* and *h V. Cat.* No. 2413 is *C.G.* 671, and is a single star. Cf. *P.M.* pp. xcvi. and 233.

2415. Σ 837. Add *Rej.* Cf. No. 14.

2416. Σ[1] (672) c.g. Entered in the *C.G.* as "vicina ad 835." The star is single.

2424. *h* 2292. This is identical with No. 2389, the place of the latter star being the correct one.

2434. The star does not exist. *M.M.* p. xxxv. *P.M.* p. cxv.

2435*. Σ 843. Add *Rej.* Cf. No. 67.

2436. Σ 842. Add *Rej.* Cf. No. 14.

2441. Σ[1] (676) c.g. Entered in the *C.G.* as "vicina ad 846;" it is, however, a single star.

2446. The N.P.D. should be 87° 51′. Cf. *P.M.* p. xcvii.

## *Note on the Phenomenon known as "Shadow Bands."*

By Dr. R. Copeland.

I had read accounts of the "Shadow Bands" sometimes seen during solar eclipses, in the *Astronomische Nachrichten*, Nos. 1921 and 1922, and elsewhere, but had not the slightest idea that any kindred phenomenon could occur at sunset or sunrise until December 18, 1879, when an appearance of the same kind was most conspicuously visible on the inner wall of the great dome at Dunecht. The bands, or rather lines, were, generally speaking, about $\frac{1}{4}$ inch broad, almost exactly horizontal, and some $2\frac{1}{2}$ inches apart. They were produced by the last rays of the Sun then setting behind the smooth horizontal ridge of the Hill of Fare, five miles distant. The lines ascended at the rate of a foot and a half in a second, and had a distinct quivering motion. The lines were seen by Herr Lohse (the assistant astronomer) and by the writer. An account of them was published in a recent No. of *Nature*.

The bands having been once seen were naturally looked for on subsequent occasions. Their occurrence proved to be very uncertain and variable. There are very fine sunsets which do not give a trace of them even on the whitest surface most completely screened from any but the direct light of the Sun. At other times the lines are short, crooked and disconnected, *descending* as well as ascending, forming and disappearing within a few seconds.

On January 13, 1880, when the Sun was setting behind the slope of Greymore (one of the summits of the Hill of Fare) the profile of which is inclined about 20° to the horizon, the northern end being the higher, Herr Lohse saw the dark lines running *downwards* for about 20 seconds. The apparently regular intervals were about half an inch broad, while the thickness of the lines was, roughly, $\frac{1}{8}$ inch. The motion was about one foot in four or five seconds. The lines were not horizontal but parallel to the slope behind which the Sun was disappearing. The lines became visible on a sheet of white paper in a room with one closed plate-glass window when about two-thirds of the Sun's disk had disappeared. Prior to this for a short time there was an irregular ripple-like mixture of light and shade, which also had a decided downward motion.

On February 3, 1880, the writer saw the lines much broken and disconnected, some ascending, a greater number descending, while a few formed and hovered on the screen, disappearing in a few seconds.

One appearance may be mentioned, as it gives what seems to be a clue to the origin of the lines. Most people have noticed the small crescent-shaped images of the partially eclipsed Sun formed by its rays passing through foliage or other imperfect obstruction. In the same way when the disk of the setting Sun is reduced to a mere line and its light falls through the branches of a leafless tree the partial images so formed are reduced to straight lines

separated by straight dark bands parallel to the apparent horizon. This occurs whatever may be the average direction of the branches.

In the case of lines formed by the crest of a distant hill (or the Moon's limb) it is assumed that irregularities in the densities of the various strata of the intervening air form the requisite obstructions, and as these irregularities vary from instant to instant, so do the lines move or change. When the source of light subtends a very small angle we have scintillation. From this point of view the dark lines are a phenomenon of scintillation projected on a screen. This hypothesis readily explains the coloured fringes of the eclipse shadow bands seen by some observers.

As seen from the neighbourhood of the Observatory at Dunecht, the Sun sets behind one or other part of the treeless ridge of the Hill of Fare for about nine weeks before and after the winter solstice. For the rest of the year the Sun disappears or reappears behind a wooded horizon far less favourable to the production of a regular phenomenon of the character described. The Sun being now beyond the limits mentioned, these notes are laid before the Society in the hope that some member who resides in a favourable locality may be on the look-out for the bands. It is at least favourable, if not essential, to their visibility that the screen on which they are formed should be protected from side light, and that the observer should be very close to the screen.

A couple of white screens making a known angle with each other and with the horizon, exposed in a room with an open window but with partially closed shutters, would probably be a good arrangement.

On comparing the above with the account of the eclipse shadow bands at p. 47, *Mem. Royal Astronomical Society*, vol. xliv., it occurs to the writer that the apparent rotation of the bands about a distant centre there described was most probably an effect of perspective, just as furrows transverse to a line of railway appear to rotate before the eyes of a traveller. In future eclipses it may be well to see if the bands rotate about *two* opposite centres, or vanishing points.

*Dunecht Observatory,*
1880, *March* 6.

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*Note on the Photographic Diameter of the Moon.* By E. Neison, Esq.

In his Note in the *Monthly Notices* for January 1880 (page 167) Prof. Pritchard adduces complete evidence of the accord between the measures made by himself and by the two assistants of the Oxford Observatory. It is with great satisfaction that I learn that the lunar photographs taken at Oxford will permit of such sharp and accordant measures being made. The difference $0''{\cdot}56$ between the separate results of Mr. Jenkins and Mr. Plummer is insignificant.

I would, however, point out that it is impracticable by means of measuring any single photograph to determine the error introduced through the irregularities on the Moon's limb; so that the evidence adduced by Prof. Pritchard fails to affect the justice of my criticism.

If these irregularities were of the nature of pointed peaks at intervals projecting in the sky, then Prof. Pritchard would be correct. But, in truth, they are great elevations extending along the limb for hundreds of miles at a time, so that it is indifferent from what point you measure: in every case you have a systematic error.

It is this systematic error which is so dangerous; the mere presence of peaks at intervals would give rise to errors which would disappear in the mean.

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*On some Changes in the Markings of Mars, since the Opposition of* 1877. By N. E. Green, Esq.

The details of *Mars* were carefully scrutinised during the past Opposition, in order to identify as far as possible the forms observed at Madeira in 1877, and to detect any changes that might have occurred since that date.

The atmosphere of St. John's Wood has not been favourable, few evenings afforded good views, and even on favourable occasions, either the atmosphere of *Mars* or its brilliant reflection made definition especially trying to the eye. The best views were obtained on nights when *Saturn* was almost put out by fog, which, reducing the glare of *Mars*, served only to make its features more distinctly visible.

However, several drawings were made, the details of which confirm by far the greatest part of the map appended to the drawings of *Mars* in the Memoirs. The various markings have been identified point by point. Indeed, with the exception of a few minute forms, the whole has been re-observed, and where the London air has failed, Professor Niesten at Brussels, Mr. Burton near Dublin, and Mr. Denning at Southampton, have succeeded in drawing just those parts which required confirmation.

Professor Niesten has forwarded a very valuable series of 16 drawings made during the last Opposition, and Mr. Burton has been particularly fortunate in securing some very exact views of the neighbourhood of Terby Sea.

The intention of this Note is to point out a few changes in the surface markings, some of which have special interest, when the drawings of 1879 are compared with those of previous Oppositions. One of these changes is the appearance during November and December 1879 of a band of light in latitude 20° S. extending from longitude 260° to 360°, uniting in one long line of light Dreyer, Hirst and Phillips Islands. To the east of

Phillips Island this band of light turned towards the equator, and, passing between Dawes Forked Bay and Burton Bay, formed a connection with Beer Continent. The interest connected with this appearance is that it repeats what was seen by Beer and Mädler in 1830, by Lockyer in 1862, and Kaiten in 1864, whereas at Madeira in 1877 this portion of the planet's surface was more or less filled with half tint, on which the islands were but indistinctly seen. This was especially the case with Phillips Island, and the space between Dawes' Forked Bay and Burton Bay always appeared dark enough to continue the equatorial band as an unbroken effect of shade.

Near Noble Cape, during the last and some previous oppositions, a line of light has been occasionally seen, connecting this form with Webb Land; but here also there was the most definite filling up of half tint during the observations made at Madeira.

These temporary observations of the darker portions of the surface are amongst the most frequent phenomena of the planet, and will account for many discrepancies in the drawings of different observers.

One of the most remarkable instances of obscuration and reappearance is that of the dark mark to the north of Terby Sea known in Mr. Proctor's map as Dawes Sea. This form was drawn by Dawes, Lockyer and Kaiten, but in the Opposition of 1877 was certainly invisible. A most careful and persevering search was made for it at Madeira, with all powers, and on the best occasions, and, with the exception of a faint elongated streak of shade, nothing could be seen. This mark has returned in all its previous definite character during the Opposition just past.

With regard to the dark canals so sharply depicted by Professor Schiaparelli, both Mr. Burton and myself have seen traces of them, and in one instance there is very fair agreement in position, but it is hardly safe to regard them as belonging to the permanent markings, for if all the dark lines which have been seen by observers were filled in on a single map the greatest confusion would ensue.

It is possible that some of these lines may be the boundaries of faint tones of shade, so delicate that they escape the notice of any but a well-trained eye, or they may be spaces between veil-like masses of atmosphere; in either case their position would be variable.

These observations would lead us to regard the larger dark marks as the most permanent features of the planet, but subject to partial obliteration, or even long-continued disappearance, from the imposition of some lighter cloud-like material.